THE

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The Psychological Review.

THE EXPERIENCE OF ACTIVITY.[1]

BY PROFESSOR WILLIAM JAMES,
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Brethren of the Psychological Association:

In casting about me for a subject for your President this year to talk about it has seemed to me that our experiences of activity would form a good one; not only because the topic is so naturally interesting, and because it has lately led to a good deal of rather inconclusive discussion, but because I myself am growing more and more interested in a certain systematic way of handling questions, and want to get others interested also, and this question strikes me as one in which, although I am painfully aware of my inability to communicate new discoveries or to reach definitive conclusions, I yet can show, in a rather definite manner, how the method works.

The way of handling things I speak of, is, as you already will have suspected, that known sometimes as the pragmatic method, sometimes as humanism, sometimes as Deweyism, and in France, by some of the disciples of Bergson, as the Philosophie nouvelle. Professor Woodbridge's *Journal of Philosophy* seems unintentionally to have become a sort of meeting place for those who follow these tendencies in America. There is only a dim identity among them; and the most that can be said at present is that some sort of gestation seems to be in the atmosphere, and that almost any day a man with a genius for finding the right word for things may hit upon some unifying

[1] President's Address before the American Psychological Association, Philadelphia Meeting, December, 1904.

and conciliating formula that will make so much vaguely similar aspiration crystallize into more definite form.

I myself have given the name of 'radical empiricism' to that version of the tendency in question which I prefer; and I propose, if you will now let me, to illustrate what I mean by radical empiricism, by applying it to activity as an example, hoping at the same time incidentally to leave the general problem of activity in a slightly — I fear very slightly — more manageable shape than before.

Mr. Bradley calls the question of activity a scandal to philosophy, and if one turns to the current literature of the subject — his own writings included — one easily gathers what he means. The opponents cannot even understand one another. Mr. Bradley says to Mr. Ward: "I do not care what your oracle is, and your preposterous psychology may here be gospel if you please; * * * but if the revelation does contain a meaning I will commit myself to this: either the oracle is so confused that its signification is not discoverable, or, upon the other hand, if it can be pinned down to any definite statement, then that statement will be false."[1] Mr. Ward in turn says of Mr. Bradley: "I cannot even imagine the state of mind to which his description applies. * * * It reads like an unintentional travesty of Herbartian Psychology by one who has tried to improve upon it without being at the pains to master it." Münsterberg excludes a view opposed to his own by saying that with any one who holds it a *Verständigung* with him is "*grundsätzlich ausgeschlossen*"; and Royce, in a review of Stout:[2] hauls him over the coals at great length for defending 'efficacy' in a way which I, for one, never gathered from reading him, and which I have heard Stout himself say was foreign to the intention of his text.

In these discussions distinct questions are habitually jumbled and different points of view are talked of *durcheinander*. There is a psychological question: "Have we perceptions of activity? and if so, what are they like, and when and where do we have them?" There is a metaphysical question: "Is there a *fact* of activity? and if so, what idea must we frame of it? What

[1] Appearance and Reality, p. 117. — Obviously written *at* Ward, though Ward's name is not mentioned.

[2] *Mind*, N. S., VI., 379.

is it like? and what does it do, if it does anything?" and finally there is a logical question: "Whence do we *know* activity? By our own feelings of it solely? or by some other source of information?" Throughout page after page of the literature one knows not which of these questions is before one; and mere description of the surface show of experience is proferred as if it implicitly answered every one of them. No one of the disputants, moreover, tries to show what pragmatic consequences his own view would carry, or what particular practical differences it would make if his adversary's were triumphant.

It seems to me that if radical empiricism be good for anything, it ought, with its pragmatic method and its principle of pure experience, to be able to avoid such tangles, or at least to simplify them somewhat. The pragmatic method starts from the postulate that there is no difference of truth that doesn't make a difference of fact somewhere; and it seeks to determine the meaning of all differences of opinion by making the discussion lead as soon as possible to some practical issue. The principle of pure experience is also a postulate. Nothing shall be admitted as fact, it says, except what can be experienced at some definite time by some experient; and for every feature of fact ever so experienced, a definite place must be found somewhere in the final system of reality. In other words: Everything real must be experienced, and every kind of thing experienced must somewhere be real.

Armed with these rules of method let us see what face the problems of activity present to us.

By the principle of pure experience, either the word 'activity' must have no meaning at all, or else the original type and model of what it means must lie in some concrete kind of experience that can be definitely pointed out. Whatever ulterior judgments we may eventually come to make regarding activity, *that sort* of thing will be what the judgments are about. The first thing to do then is to ask where in the stream of experience we seem to find what we speak of as activity. What we are to think of the activity thus found will be a later question.

Now it is obvious that we are tempted to affirm activity

wherever we find anything *going on*. Taken in the broadest sense, any apprehension of something *doing*, is an experience of activity. Were our world describable only by the words 'nothing happening,' 'nothing changing,' we should unquestionably call it an 'inactive' world. Elementary activity then, as we may call it, means the bare fact of event or change. 'Change taking place' is a unique content of experience, one of those 'conjunctive relations' between its successive parts which radical empiricism seeks so earnestly to rehabilitate and preserve. The sense of activity is thus in the broadest and vaguest way synonymous with the sense of 'life.' We should feel our own subjective life at least, even in noticing and proclaiming an otherwise inactive world. Our own reaction on its monotony would be the one thing experienced there in the form of an event.

This seems to be what certain writers have in mind when they insist that for an experient to be at all is to be active. It seems to justify, or at any rate to explain, Mr. Ward's expression that we *are* only as we are active,[1] for we *are* only as experients; and it rules out Mr. Bradley's contention that "there is no original experience of anything like activity." What we ought to say about activities thus elementary, whose they are, what they effect, or whether indeed they effect anything at all — these are later questions to be answered only when the field of experience is enlarged.

Elementary activity would thus be predicable, though there were no definite direction, no actor, and no aim. Mere restless zigzag movement, or a wild *Ideenflucht*, or *Rhapsodie der Wahrnehmungen*, as Kant would say, would constitute an active as distinguished from an inactive world.

But in this actual world of ours, as it is given, a part at least of the activity comes with definite direction; it comes with desire and sense of goal; it comes complicated with resistances which it overcomes or succumbs to, and with the efforts which the feeling of resistance so often provokes; and it is in complex experiences like these that the notions of distinct agents, and of

[1] *Naturalism and Agnosticism*, II., 245. One thinks naturally of the peripatetic *actus primus* and *actus secundus* here.

passivity as opposed to activity arise. Here also the notion of causal efficacy comes to birth. Perhaps the most elaborate work ever done in descriptive psychology has been the analysis by various recent writers of the more complex activity-situations.[1] In their descriptions, exquisitely subtle some of them,[2] the activity appears as the *gestaltqualität* or the *fundirte inhalt* (or as whatever else you may please to call the conjunctive form) which the content falls into when we experience it in the ways which the describers set forth. Those factors in those relations are what we mean by activity-situations; and to the possible enumeration and accumulation of their circumstances and ingredients there would seem to be no natural bound. Every hour of human life could contribute to the picture gallery; and this is the only fault that one can find with such descriptive industry — where is it going to stop? Ought we to listen forever to verbal pictures of what we have already in concrete form in our own breasts?[3] They never take us off the superficial plane. We knew the facts already — less spread out and separated, to be sure — but we knew them still. We always felt our own activity, for example, as 'the expansion of an idea with which our Self is identified, against an obstacle'; and the following out of such a definition through a multitude of cases elaborates the obvious so as to be little more than an exercise in synonymic speech.

All the descriptions have to trace familiar outlines, and to use familiar terms. The activity is, for example, attributed

[1] I refer to such descriptive work as Ladd's (*Psychology, Descriptive and Explanatory*, Part I., Chap. V., Part II., Chap. XI., Part III., Chaps. XXV. and XXVI.); as Sully's (*The Human Mind*, Part V.); as Stout's (*Analytic Psychology*, Book I., Chap. VI., and Book II., Chaps. I., II. and III.); as Bradley's (in his long series of analytic articles on Psychology in *Mind*); as Titchener's (*Outline of Psychology*, Part I., Chap. VI.); as Shand's (*Mind*, N. S., III., 449; IV., 450; VI., 289); as Ward's (*Mind*, XII., 67 ff., and 564); as Loveday's (*Mind*, N. S., X., 455); as Lipps's (Vom Fühlen, Wollen und Denken, 1902, Chaps. II., IV., VI.); and as Bergson's (*Revue Philosophique*, LIII., 1) — to mention only a few writings which I immediately recall.

[2] Their existence forms a curious commentary on Prof. Münsterberg's dogma that will-attitudes are not describable. He himself has contributed in a superior way to their description, both in his *Willenshandlung*, and in his *Grundzüge*, Part II., Chap. IX., § 7.

[3] I ought myself to cry *peccavi*, having been a voluminous sinner in my own chapter on the will.

either to a physical or to a mental agent, and is either aimless or directed. If directed it shows tendency. The tendency may or may not be resisted. If not, we call the activity immanent, as when a body moves in empty space by its momentum, or our thoughts wander at their own sweet will. If resistance is met, two agents complicate the situation. If now, in spite of resistance, the original tendency continues, effort makes its appearance, and along with effort, strain or squeeze. Will, in the narrower sense of the word then comes upon the scene, whenever, along with the tendency, the strain and squeeze are sustained. But the resistance may be great enough to check the tendency, or even to reverse its path. In that case, we (if 'we' were the original agents) are overpowered. The phenomenon turns into one of tension simply, or of necessity succumbed-to, according as the opposing power is only equal, or is superior to ourselves.

Whosoever describes an experience in such terms as these describes an experience *of* activity. If the word have any meaning it must denote what there is found. *There* is complete activity in its original and first intention. What it is 'known-as' is what there appears. The experiencer of such a situation possesses all that the idea contains. He feels the push, the obstacle, the will, the strain, the triumph or the passive giving up, just as he feels the time, the space, the swiftness or intensity, the movement, the weight and color, the pain and pleasure, the complexity, or whatever remaining characters the situation may involve. He goes through all that ever can be imagined where activity is supposed. If we suppose activities to go on outside of our experience it is in forms like these that we must suppose them, or else give them some other name; for the word 'activity' has no content save these experiences of process, obstruction, striving, strain, or release, ultimate *qualia* as they are of the life given us to be known.

Were this the end of the matter, one might think that whenever we had successfully lived through an activity-situation we should have to be permitted, without provoking contradiction, to say that we had been really active, that we had met real resistance and had really prevailed. Lotze somewhere says

that to be an entity all that is necessary is to *gelten* as an entity, to operate, or be felt, experienced, recognized, or in any way realized, as such. In our activity-experiences the activity assuredly fulfils Lotze's demand. It makes itself *gelten*. It is witnessed at its work. No matter what activities there may really be in this extraordinary universe of ours, it is impossible to conceive of any one of them being either lived through or authentically known otherwise than in this dramatic shape of something sustaining a felt purpose against felt obstacles and overcoming or being overcome. What 'sustaining' means here is clear to anyone who has lived through the experience, but to no one else; just as 'loud,' 'red,' 'sweet,' mean something only to beings with ears, eyes and tongues. The *percipi* in these originals of experience is the *esse;* the curtain is the picture. If there is anything hiding in the background, it ought not to be called activity, but should get itself another name.

This seems so obviously true that one might well experience astonishment at finding so many of the ablest writers on the subject flatly denying that the activity we live through in these situations is real. Merely to feel active is not to be active, in their sight. The agents that appear in the experience are not real agents, the resistances do not really resist, the effects that appear are not really effects at all.[1] It is evident from this that

[1] *Verborum gratiâ:* "The feeling of activity is not able, quā feeling, to tell us anything about activity" (Loveday: *Mind*, N. S., X., 463); "A sensation or feeling or sense of activity * * * is not, looked at in another way, a feeling of activity at all. It is a mere sensation shut up within which you could by no reflection get the idea of activity. * * * Whether this experience is or is not later on a character essential to our perception and our idea of activity, it, as it comes first, is not in itself an experience of activity at all. It, as it comes first, is only so for extraneous reasons and only so for an outside observer" (Bradley, *Appearance and Reality*, 2d edition, p. 605); "In dem Tätigkeitsgefühle liegt an sich nicht der geringste Beweis für das Vorhandensein einer psychischen Tätigkeit" (Münsterberg: Grundzüge, etc., p. 67). I could multiply similar quotations and would have introduced some of them into my text to make it more concrete, save that the mingling of different points of view in most of these author's discussions (not in Münsterberg's) make it impossible to disentangle exactly what they mean. I am sure in any case, to be accused of misrepresenting them totally, even in this note, by omission of the context, so the less I name names and the more I stick to abstract characterization of a merely possible style of opinion, the safer it will be. And apropos of misunderstandings, I may add to this note a complaint on my own account. Professor Stout,

mere descriptive analysis of any one of our activity-experiences is not the whole story, that there is something still to tell *about* them that has led such able thinkers to conceive of a *Simon pure* activity, of an activity *an sich*, that does, and doesn't merely appear to us to do, and compared with which all this phenomenal activity is but a specious sham.

The metaphysical question opens here; and I think that the state of mind of one possessed by it is often something like this: "It is all very well," we may imagine him saying, "to talk about certain experience-series taking on the form of activities, just as they might take on musical or geometric forms. Suppose that they do so; suppose we feel a will to stand a strain. Does our feeling do more than *record* the fact? The will's activity meanwhile, is the *doing* of the fact; and what is the doing made of before the record is made. What in the will *enables* it to act? And these trains of experience themselves, in which activities appear, what makes them *go* at all? Does the activity in one bit of experience bring the next bit into being?

in the excellent chapter on 'Mental Activity,' in Vol. I., of his *Analytic Psychology*, takes me to task for identifying spiritual activity with certain muscular feelings and gives quotations to bear him out. They are from certain paragraphs on 'the Self,' in which my attempt was to show what the central nucleus of the activities that we call 'ours' is. I found it in certain intracephalic movements which we habitually oppose, as 'subjective,' to the activities of the transcorporeal world. I sought to show that there is no direct evidence that we feel the activity of an inner spiritual agent as such (I should now say the activity of 'consciousness' as such, see my paper 'Does consciousness exist?' in the *Journal of Philosophy*, Vol. I., p. 477). There are in fact three distinguishable 'activities' in the field of discussion: the 'elementary' activity involved in the mere *that* of experience, in the fact that *something* is going on, and the farther specification of this *something* into two *whats*, an activity felt as 'ours,' and an activity ascribed to objects. Stout, as I apprehend him, identifies 'our' activity with that of the total experience-process, and when I circumscribe it as a part thereof, accuses me of treating it as a sort of external appendage to itself (pp. 162–3), as if I 'separated the activity from the process which is active.' But all the processes in question are active, and their activity is inseparable from their being. It was in my book only a question of *which* activity deserved the name of 'ours.' So far as we are 'persons,' and contrasted and opposed to an 'environment,' movements in our body figure as our activities; and I am unable to find any other activities that are ours in this strictly personal sense. There is a wider sense in which the whole 'choir of heaven and furniture of the earth' and their activities, are ours, for they are our 'objects.' But 'we' are here only another name for the total process of experience, another name for all that is, in fact; and I was dealing with the

As an empiricist you cannot say so, for you have just declared activity to be only a kind of conjunctive relation experienced between bits of experience already made. But what made them at all ? What propels experience *überhaupt* into being ? *There* is the activity that *operates;* the activity *felt* is only its superficial sign."

To the metaphysical question, popped upon us in this way, I must pay serious attention ere I end my remarks, but, before doing so, let me show that without leaving the immediate reticulations of experience, or asking what makes activity itself act, we still find the distinction between less real and more real activities forced upon us, and are driven to much soul-searching on the purely phenomenal plane.

We must not forget, namely, in talking of the ultimate character of our activity-experiences, that each of them is but a portion of a wider world, one link in the vast chain of processes of which history is made. Each partial process, to him who lives through it, defines itself by its origin and its goal; but to an observer with a wider mind-span who should live outside of it, that goal would appear but as a provisional halting place,

personal and individualized self exclusively in the passages with which Professor Stout finds fault.

The individualized self, which I believe to be the only thing properly called self, is a part of the content of the world experienced. The world experienced (otherwise called the 'field of consciousness') comes at all times with our body as its center, center of vision, center of action, center of interest. Where the body is is 'here'; when the body acts is 'now'; what the body touches is 'this'; all other things are 'theres' and 'thens' and 'thats.' These words of emphasized position imply a systematization of things with reference to a focus of action and interest which lies in the body; and the systematization is now so instinctive (was it ever not so?) that no developed or active experience exists for us at all except in that ordered form. So far as 'thoughts' and 'feelings' can be active, their activity terminates in the activity of the body, and only through first arousing its activities can they begin to change those of the rest of the world. The body is the storm center, the origin of coördinates, the constant place of stress in all that experience-train. Everything circles round it, and is felt from its point of view. The word 'I,' then, is primarily a noun of position, just like 'this' and 'here.' Activities attached to 'this' position have prerogative emphasis, and, if functions have feelings, must be felt in a peculiar way. The word 'my' designates the kind of emphasis. I see no inconsistency whatever, in defending on the one hand 'my' activities as unique and opposed to those of outer nature, and on the other hand in affirming, after introspection, that they consist in movements in the head. The 'my' of them is the emphasis, the feeling of perspective-interest in which they are dyed.

and the subjectively felt activity would be seen to continue into objective activities that led far beyond. We thus acquire a habit, in discussing activity-experiences, of defining them by their relation to something more. If an experience be one of narrow span, it will be mistaken as to what activity it is and whose. You think that *you* are acting and you are only obeying someone's push. You think you are doing *this*, but you are doing something of which you do not dream. For instance, you think you are but drinking this glass; but you are really creating the liver-cirrhosis that will end your days. You think you are just driving this bargain, but, as Stevenson says somewhere, you are laying down a link the policy of mankind.

Generally speaking, the *ultimate outcome* of an activity is regarded by an onlooker as what it is more really doing; and *the most previous agent* ascertainable, being the first source of action, is regarded as the most real agent in the field. The others but transmit his impulse; on him we put responsibility; we name him when one asks us 'Who's to blame?'

But the most previous agents ascertainable, instead of being of longer span, are often of much shorter span than the activity in view. Brain-cells are our best example. My brain-cells are believed to excite each other from next to next (by contiguous transmission of katabolic alteration, let us say) and to have been doing so long before this present stretch of lecturing-activity on my part began. If any one cell-group stops its activity, the lecturing will cease or show disorder of form. *Cessante causa, cessat et effectus* — does not this look as if the short-span brain activities were the more real activities, and the lecturing activities on my part only their effects? Moreover, as Hume so clearly pointed out, in my mental activity-situation the words physically to be uttered are represented as the activity's immediate goal. These words, however, cannot be uttered without intermediate physical processes in the bulb and vagi nerves, which processes nevertheless fail to figure in the mental activity-series at all. That series, therefore, since it leaves out vitally real steps of action, cannot represent the real activities. It is something purely subjective; the *facts* of activity are elsewhere.

The real facts of activity that have in point of fact been sys-

tematically pleaded for by philosophers have, so far as my information goes, been of three principal types.

The first type takes a consciousness of wider time-span than ours to be the vehicle of the more real activity. Its will is the agent, and its purpose is the action done.

The second type assumes that 'ideas' struggling with one another are the agents, and that the prevalence of one set of them is the action.

The third type believes that nerve-cells are the agents, and that resultant motor discharges are the acts achieved.

Now if we must de-realize our immediately felt activity-situations for the benefit of either of these types of substitute, we ought to know what the substitution practically involves. *What practical difference ought it to make if*, instead of saying naïvely that 'I' am active now in delivering this address, I say that *a wider thinker is active*, or that *certain ideas are active*, or that *certain nerve cells are active*, in producing the result?

This would be the pragmatic meaning of the three hypotheses. Let us take them in succession in seeking a reply.

If we assume a wider thinker, it is evident that his purposes envelope mine. I am really lecturing *for* him; and although I cannot surely know to what end, yet if I take him religiously, I can trust it to be a good end, and willingly connive. I can be happy in thinking that my activity transmits his impulse, and that his ends prolong my own. So long as I take him religiously, in short, he does not de-realize my activities. He tends rather to corroborate the reality of them, so long as I believe both them and him to be good.

When now we turn to ideas, the case is different, inasmuch as ideas are supposed by the association psychology to influence each other only from next to next. The 'span' of an idea or pair of ideas, is assumed to be much smaller instead of being larger than that of my total conscious field. The same results may get worked out in both cases, for this address is being given anyhow. But the ideas supposed to 'really' work it out had no prevision of the whole of it; and if I was lecturing for the Absolute in the former case, so, by parity of reasoning, are my ideas now lecturing for me, that is, accomplishing unwit-

tingly a result which I approve and adopt. But, when this passing lecture is over, there is nothing in the bare notion that ideas have been its agents that would seem to guarantee that my present purposes in lecturing will be prolonged. *I* may have ulterior developments in view; but there is no certainty that my ideas as such will wish to, or be able to, work them out.

The like is true if nerve-cells be the agents. The activity of a nerve-cell must be conceived of as a tendency of exceedingly short reach, an 'impulse' barely spanning the way to the next cell — for surely that amount of actual 'process' must be 'experienced' by the cells if what happens between them is to deserve the name of activity at all. But here again the *gross* result, as *I* perceive it, is indifferent to the agents, and neither wished or willed or foreseen. Their being agents *now* gives me no guarantee that like results will recur again. In point of fact all sorts of other results do occur. My mistakes, impotencies, perversions, mental obstructions, and frustrations generally, are also results of the activity of cells. Although these are letting me lecture now, on other occasions they make me do things that I would willingly not do.

The question *Whose is the real activity?* is thus tantamount to the question *What will be the actual results?* Its interest is dramatic; how will things work out? If the agents are of one sort, one way; of another sort, otherwise as well. The pragmatic meaning of the various alternatives in short is great. It makes no merely verbal difference which opinion we take up.

You see it is the old dispute come back! Materialism and teleology; elementary short-span actions summing themselves 'blindly,' or far foreseen ideals coming with effort into act.

Naïvely we believe, and humanly and dramatically we like to believe, that activities both of wider and of narrower span are at work in life together, that both are real, and that the long-span tendencies yoke the others in their service, encouraging them in the right direction, and damping them when they tend in other ways. But how to represent clearly the *modus operandi* of such steering of small tendencies by large ones is a problem which metaphysical thinkers will have to ruminate upon for many years to come. Even if such control should eventually

grow clearly picturable, the question how far it is successfully exerted in this actual world can only be answered by investigating the details of fact. No philosophic knowledge of the general nature and constitution of tendencies, or of the relation of larger to smaller ones, can help us to predict which of all the various competing tendencies that interest us in this universe are likeliest to prevail. We know as an empirical fact that far-seeing tendencies often carry out their purpose, but we know also that they are often defeated by the failure of some contemptibly small process on which success depends. A little thrombus in a statesman's meningeal artery will throw an empire out of gear. I can therefore not even hint at any solution of the pragmatic issue. I have only wished to show you that that issue is what gives the real interest to all inquiries into what kinds of activity may be real.

I said a while back that I should return to the 'metaphysical' question before ending; so, with a few words about that, I will now close my remarks.

In whatever form we hear this question propounded, I think that it always arises from two things, a belief that *causality* must be exerted in activity, and a wonder as to how causality is made. If we take an activity situation at its face-value it seems as if we caught *in flagrante delicto* the very power that makes facts come and be. I now am eagerly striving, for example, to get this truth which I seem half to perceive, into words which shall make it show more clearly. If the words come, it will seem as if the striving itself had drawn or pulled them into actuality out from the state of merely possible being in which they were. How is this feat performed? How does the pulling *pull?* How do I get my hold on words not yet existent, and when they come by what means have I *made* them come? Really it is the problem of creation; for in the end the question is: How do I make them *be*? Real activities are those that really make things be, without which the things are not, and with which they are there. Activity, so far as we merely feel it, on the other hand, is only an impression of ours, one may say; and an impression is, for all this way of thinking, only a shadow of another fact.

Arrived at this point, I can do little more than indicate the

principles on which, as it seems to me, a radically empirical philosophy is obliged to rely in handling such a dispute.

If there *be* real creative activities in being, radical empiricism must say, somewhere they must be immediately lived. Somewhere the *that* of efficacious causing and the *what* of it must be experienced in one, just as the what and the that of 'cold' are experienced in one whenever a man has the sensation of cold here and now. It boots not to say that our sensations are fallible. They are indeed; but to see the thermometer contradict us when we say 'it is cold' does not abolish cold as a specific nature from the universe. Cold is in the arctic circle if not here. Even so, to feel that our train is moving when the train beside our window moves, to see the moon through a telescope come twice as near, or to see two pictures as one solid when we look through a stereoscope at them, leaves motion, nearness and solidity still in being — if not here, yet each in its proper seat elsewhere. And wherever the seat of real causality *is*, as ultimately known 'for true' (in nerve-processes, if you will, that cause our feelings of activity as well as the movements which these seem to prompt), a philosophy of pure experience can consider the real causation as no other *nature* of thing than that which even in our most erroneous experiences appears to be at work. Exactly what appears there is what we *mean* by working, though we may later come to learn that working was not exactly *there*. Sustaining, persevering, striving, paying with effort as we go, hanging on, and finally achieving our intention — this *is* action, this *is* effectuation in the only shape in which, by a pure experience-philosophy, the wherabouts of it anywhere can be discussed. Here is creation in its first intention, here is causality at work.[1] To treat this offhand as the bare surface of a world

[1] Let me not be told that this contradicts a recent article of mine, 'Does Consciousness Exist?' in the *Journal of Philosophy* for September 1, 1904 (see especially page 489), in which it was said that while 'thoughts' and 'things' have the same natures, the natures work 'energetically' on each other in the things (fire burns, water wets, etc.) but not in the thoughts. Mental activity-trains are composed of thoughts, yet their members do work on each other, they check, sustain, and introduce. They do so when the activity is merely associational as well as when effort is there. But, and this is my reply, they do so by other parts of their nature than those that energize physically. One thought in every developed activity-series is a desire or thought of purpose, and

whose real causality is a more solid way of action hidden in the cubic deeps, is for the more empirical way of thinking, only animism is another shape. You explain your given fact by a 'principle,' but the principle itself, when you look clearly at it, turns out to be nothing but a previous little spiritual edition of the fact. Away from that one and only *kind* of fact your mind, considering causality, can never get.[1]

I conclude then that real effectual causation as an ultimate nature, as a 'category,' if you like, of reality, is just what we feel it to be, just that kind of conjunction which our own activity-series reveal. We have the whole butt and being of it in our hands; and the healthy thing for philosophy is to leave off grubbing underground for what effects effectuation, or what makes action act, and to try to solve the concrete questions of where effectuation in this world is located, of which things are the true causes there, and of what the more remote effects consist.

all the other thoughts acquire a feeling tone from their relation of harmony or oppugnancy to this. The interplay of these secondary tones (among which 'interest,' 'difficulty' and 'effort' figure) runs the drama in the mental series. In what we term the physical drama these qualities play absolutely no part. The subject needs careful working out; but I can see no inconsistency.

[1] I have found myself more than once accused in print of being the assertor of a metaphysical principle of activity. Since literary misunderstandings retard the settlement of problems, I should like to say that such an interpretation of the pages I have published on Effort and on Will is absolutely foreign to what I meant to express. I owe all my doctrines on this subject to Renouvier; and Renouvier, as I understand him, is (or at any rate then was) an out and out phenomenist, a denier of 'forces' in the most strenuous sense. Single clauses in my writing, or sentences read out of their connection, may possibly have been compatible with a transphenomenal principle of energy; but I defy anyone to show a single sentence which, taken with its context, should be naturally held to advocate that view. The misinterpretation probably arose in the first instance from my defending (after Renouvier) the indeterminism of our efforts. 'Free will' was supposed to involve a supernatural agent. As a matter of plain history the only 'free will' I have ever thought of defending is the character of novelty in fresh activity-situations. If an activity-process is the form of a whole 'field of consciousness,' and if each field of consciousness is not only in its totality unique (as is now commonly admitted) but has its elements unique (since in that situation they are all dyed in the total) then novelty is perpetually entering the world and what happens there is not pure *repetition*, as the dogma of the literal uniformity of nature requires. Activity-situations come in short each with an original touch. A 'principle' of free will if there were one, would doubtless manifest itself in such phenomena, but I never saw, nor do I now see, what the principle could do except rehearse the phenomenon beforehand, or why it ever should be invoked.

From this point of view the greater sublimity traditionally attributed to the metaphysical inquiry, the grubbing inquiry, entirely disappears. If we could know what causation really is in itself, the only use of the knowledge would be to help us to recognize an actual cause when we had one, and so to track the actual causal operations out. The mere abstract inquiry into causation's hidden nature is not more sublime than any other inquiry equally abstract. Causation inhabits no more sublime level than anything else. It lives, apparently, in the dirt as well as in the Absolute, or as in man's unconquerable mind. The worth and interest of the world consists not in its elements, be these elements things, or be they the conjunctions of things; it exists rather in the dramatic outcome in the whole process, and in the meaning of the stages, which the elements work out.

My colleague and master, Josiah Royce, in a page of his review of Stout's 'Analytic Psychology' in *Mind* for 1897 has some fine words on this point with which I cordially agree. I cannot agree with his separating the notion of efficacy from that of activity altogether (this I understand to be one contention of his) for activities are efficacious whenever they are real activities at all. But the inner nature both of efficacy and of activity are superficial problems, I understand Royce to say; and the only point for us in solving them would be their possible use in helping us to solve the far deeper problem of the course and meaning of the world of life. Life, says our colleague, is full of significance, of meaning, of success and of defeat, of hoping and of striving, of longing, of desire, and of inner value. It is a total presence that embodies worth. To live our own lives better in this presence is the true reason why we wish to know the elements of things; so even we psychologists must end on this pragmatic note.

The urgent problems of activity are thus more concrete. They are all problems of the true relation of longer-span to shorter-span activities. When, for example, a number of 'ideas' (to use the name traditional in psychology) grow confluent in a larger field of consciousness, do the smaller activities still coexist with the wider activities now experienced? And, if so, do the wider activities accompany the narrower ones inertly, or do

they exert control? Or do they rather utterly supplant and replace them and short-circuit their effects? Again, when a mental activity-process and a brain-cell series of activities both terminate in the same muscular movement, does the mental process steer the neural processes or not? Or, on the other hand, does it independently short-circuit their effects? Such are the questions that we must begin with. But so far am I from suggesting any definitive answer to such questions, that I hardly yet can put them clearly. They lead, however into that region of panpsychic and ontologic speculation of which Professor Bergson and our colleague Strong have lately enlarged the literature in so able and interesting a way. The results of these authors seem in many respects dissimilar, and I understand them as yet but imperfectly; but I cannot help suspecting that the direction of their work is very promising, and that they have the hunter's instinct for the fruitful trails. I earnestly recommend the study of their writings to the members of our two Associations.

THE RELATION OF PERCEPTIVE AND REVIVED MENTAL MATERIAL AS SHOWN BY THE SUBJECTIVE CONTROL OF VISUAL AFTER-IMAGES.

CONTRIBUTED FROM THE PSYCHOLOGICAL LABORATORY OF OHIO STATE UNIVERSITY.

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I.

The existence of mental content of these two sorts, is not in question here. Indeed, the fact of relation between them is not in question. There are mental processes which arise primarily by a stimulus coming to us from the environment, — stimuli impinging upon our sense organs, be they inner or outer, — and there are mental processes which seem to arise within the nervous system itself. In every act of perception also, no matter how simple it is, we seem to have involved, on the one hand an objective factor, and on the other what Professor James has called the 'ideational excitement.' There seems to be an interworking of these in every case of conscious mental acquisition. Herbart set forth the fact of this relation in his classic exposition of the apperceptive process. He showed the fact of their existence, but by his mechanical treatment of psychology he was unable to show how they coöperated in the production of new content. Professor James, in his discussion of 'The Intimate Nature of the Attention Process,'[1] gives the whole matter a very much more lucid description. 'Attentive' is in general applied to one who is experiencing, or perceiving, or cognizing, in a higher and more vivid fashion than ordinarily. Attention may therefore be assumed to exaggerate and bring into clear light the important factors in all cognition.

There are, even at the present time, recrudescences of Condillac's theory of attention. In fact the purposes of explanation are always served best where that which appears to be com-

[1] *The Principles of Psychology*, Vol. 1, pp. 434 ff.

plex, can be shown to be simple. Could one see the essence of attention as a simple inpouring of new mental material aroused by the inpouring of stimuli, — as nothing more than the exclusive and excessive use of a given avenue of sense at a given time, this indeed would be in the service of science. Ribot and Lange, in placing the emphasis they do upon the motor adaptation, as constituting the essence of the attention process, emphasize the facilitation of the incoming stimulus, and make the so-called inner factor to consist wholly in a bodily preparation. But the inner factor is more than this. It is something independent of and separate from the adaptation of the sense organ. Lange[1] maintains that the anticipatory preparation, from within, of the ideational centers, is the result, simply and solely of the motor adjustment. He would go even so far as to maintain that all imagination depends upon motor adjustment. And thus all so-called centrally-aroused mental processes are nothing more than sensations from motor organs, and the resultant feelings. This may be a too liberal interpretation of the author, but it flows from his view. It is an aim in the right direction, as it seeks simplicity; but it goes far beyond, and even contrary to, the facts. That imagination and all centrally aroused mental processes are not solely the product of motor adaptation, can be asserted unqualifiedly. That some of them are so aroused we will grant. For some mental types it may be that all are so aroused; but this is certainly not true for others. It is a hasty generalization to claim that they are. We may have motor adjustment without attention. But it may still be urged that the inner adjustment is a resultant of the motor adjustment; that it is lacking in these cases simply because the outer adjustment process fails to proceed to its natural development. But where this is a fact it already indicates that the two are less nearly the same process than they were claimed to be. And this relation is no longer thinkable when we realize the reverse fact, that we may have the ideational preparation of attention, or the revival of content without the innervation of any muscles. At least, in many persons of other than the muscular type, it seems not only possible, but the normal process, to be attentive to intellec-

[1] *Philosophische Studien*, 4: 413 ff.

tual processes without any muscular contractions, and consequently without any directly aroused kinæsthetic sensations. We think it highly probable however that there are revived kinæsthetic sensations present.

It is also highly probable that these and all revived mental processes bear a much closer relation to the physical basis of the original processes of which they are the revival, than is commonly supposed. It may even be that the peripheral sensory neurons are involved, — that the difference between this revived process and the original is really one of accompanying processes. If this is true, it is at once evident that the contentions of both sides in the attention debate are right. There may be cases of subjective attention where the objective factor is wholly represented by incipient motor processes which however fail to become anything more than tendencies to action, and so they pass for cases of attention without motor adjustment. And there may be cases of attention where the motor processes are the cue for the centrally aroused processes. The whole matter of the relation of the peripherally aroused and the centrally aroused or revived mental processes is in a very unsatisfactory state. The voluntary control of after-images offers a means of studying the relation between a centrally initiated and a peripherally aroused process.

There are four distinct lines of observation bearing on this line of work, as follows:

1. Vivid visualization of memory images.

2. Control of the color and form of idio-retinal light.

3. After-images of memory images of color, or of subjectively induced color.

4. Control of the color of visual after-images.

1. There are many classic instances of vivid visualization of color, and discussions of their relations to the so-called spectral illusions, to visual hallucinations, and to dreams proper. Notable amongst these is the account of his own experience given by Sir Isaac Newton in a letter to John Locke. This account has contained so much of suggestion for the present investigation that we shall quote it in part. Newton looked upon the image of the sun in a mirror, for a very short time, with his

right eye, and then turned his eyes toward a dark corner to observe the decay of the after-images. He says: "This I repeated a second and third time. At the third time, when the phantasm of light and colors about it were almost vanished, intending my fancy upon them to see their last appearance, I found, to my amazement that *they began to return, and by little and little, to become as lively and vivid as when I had newly looked upon the sun;* but when I ceased to intend my fancy upon them, they vanished again. After this, I found that as often as I went into the dark, and intended my mind upon them, as when a man looks earnestly to anything which is difficult to be seen, *I could make the phantasm return*, without looking any more upon the sun, and, *the oftener I made it return, the more easily I could make it return again.* And at length, by repeating this, without looking any more upon the sun, I made such an impression upon my eye, that if I looked upon the clouds, or a book, or any bright object, I saw upon it a round bright spot of light, like the sun, and, which is still stranger, *though I looked upon the sun with my right eye only, and not with my left, yet my fancy began to make an impression on my left eye*, as well as upon my right: for if I shut my right eye, and looked upon a book or the clouds with my left eye, I could see the spectrum of the sun almost as plain as with my right eye, if I did but intend my fancy a little while upon it. * * * And now, in a few hours' time, I had brought my eyes to such a pass that I could look upon no bright object with either eye, but that I saw the sun before me, so that I durst neither write nor read, but to recover the use of my eyes, shut myself up in my chamber, made dark, for three days together, and used all means to divert my imagination from the sun." By this means he began to recover the use of his eyes in three or four days. "But for some months," he says, "the spectrum of the sun began to return as often as I began to mediate upon the phenomena, even though I lay in bed at midnight with my curtains drawn."[1]

[1] This letter is given in full in King's *Life of Locke*, 1830, Vol. I., p. 40; also quoted by Sir David Brewster, *Quarterly Review*, July, 1831, 45: 341–358.

We have to do here with a strong after-image and also a very vivid memory image. It is an after-image in the early stages. It comes back involuntarily when the retinal illumination is changed, as when he looks at a cloud or a book. After-images may also be shifted from one eye to the other. This though is not a means of identifying an after-image. This behavior of after-images may really be a point of close connection between them and memory images. It is certainly a matter of the voluntary control of the after-image. Revived processes play a part in the phenomenon. Newton's description is also clearly of a memory image. His phrase 'intending my fancy upon it' is very aptly descriptive of the process of voluntary revival and intellectual attention. This 'intention of the fancy,' or direction of the attention, upon a strong retinal impression (this includes the after-image) greatly increases the vividness of the latter — brings it into great prominence in the life of the mind, and makes it far more liable to recall.

Goethe's experience of the constantly unfolding rosette, which he saw every time he closed his eyes and bent his head forward, is a case of a habitual connection between a position and a memory image. The position was the cue to set it going. It would constantly throw out new petals of different colors, but mostly red, as long as he cared to watch it. A Mrs. Haweis, one of Mr. Galton's correspondents, as a child, when it was dark, saw a flight of pink roses floating in a mass from left to right, which presently changed to a flight of sparks or gold speckles.[1] These are both cases of automatized memory images. They have started in some more or less accidental way, which the person no longer knows about. Children have many more of these vagrant images than do adults. Of the specifically willed images, those of the Bushmen drawing animals are in point.[2] They carefully place several dots, and then rapidly fill in the sketch. They evidently have a mental picture before the mind's eye.

Strong visualizers can project their mental pictures, and it sometimes becomes difficult for them to distinguish this vivid

[1] *Human Faculty*, p. 159.
[2] Galton, *Human Faculty*, p. 103.

mental imagery from the real world. More frequently though the mental image, projected, seems transparent like the reflected image from plain glass, and the real is seen through it, as the transmitted is through the reflected image. It is as if the mental image were seen in a lustrous surface through which the landscape was visible. Like the illusion of movement in Fleischl's waterfall experiment, in a way it is there, and in another way it is not there. In a way it seems real, and in a way it does not seem real. In so far as we distinguish it from the real, it is merely mental imagery; and in so far as it seems real it is hallucination.

This power of visualizing the form and color of objects, real or imagined, is one capable of cultivation. It increases rapidly when attention is directed toward it. A little systematic effort will convince anyone of the truth of this statement.[1]

2. As a more restricted field, but at the same time a part of the group of phenomena considered, we are especially interested in what Professor Ladd has called the "voluntary control of the 'Eigenlicht.'"[2] Professor Ladd, finding that the light of the retina assumed various and peculiar forms, in his case, without effort on his part, set about trying what he could do voluntarily to modify the form and the color. "This power grew rapidly, with continued practice; that is to say I was soon able, by attentively willing (compare Newton's 'intending his fancy'), for perhaps some three to five minutes, to cause a cross or a circle, or two concentric circles or some other simple figure, to appear in the retinal field." The method was simply to close the eyes, wait till all after-images had died away, and then persistently and attentively *to will* that the color mass caused by the 'Eigenlicht' should take on some particular form or should change in color. A number of students who took up the experiment at Professor Ladd's suggestion had very good success in both of these lines. It seems that any one can get some power of calling up colors and forms at will, by persistently trying to do so.

[1] See Galton, *Human Faculty*, pp. 109 and 106. See also James, *Principles of Psy.*, II., p. 66; F. Meakin, *Harvard Psy. Studies*, I., pp. 235–275; and C. S. Moore, *Harvard Psy. Studies*, I., pp. 277–306.

[2] *Direct Control of the Retinal Field*, G. T. Ladd, PSYCH. REV., 1: 51–355.

3. There are several records of visual after-images (negative and complementary) of forms and colors, which themselves have been induced voluntarily and without external stimuli, as in the cases just cited. The color, having been induced by subjective control, and then being allowed to take free course, runs into a complementary stage. G. H. Meyer[1] says that most of his subjectively induced colors, when bright, left after-images behind them when the eyes were quickly opened during their presence. Some of Ladd's students observed the same phenomenon. Binet and Féré[2] find that the persistent idea of a color develops into its complementary.

Complementary after-images of hallucinatory color impressions, induced in the hypnotic state, have been reported frequently. It is claimed that an hallucination of a red star, for example, is followed by a green after-image of the same form, and that this follows without any suggestion whatever from the hypnotizer. But this relation must, as yet, be regarded as decidedly doubtful. It is of less weight than that furnished by normal subjects. If, in either case, there is any hint of the probability of the complementary color succeeding the induced color, this would be induced as certainly and as easily as the first color. Both alike would be subjective, meaning by that non-retinal, in origin. The hypnotic subject is more liable to this than the normal because of his abnormal suggestibility. He may take a suggestion without either himself or the hypnotizer suspecting it.[3] Even Charcot was led to suppose that some of the fulfillments of his own expectations were a part of the natural process, to the extent of falling into error in description and classification of hypnotic phenomena.

If however the complementary color impression arises independently of either suggestion or volition, it at once constitutes a close connection between the memory image or induced subjective color impression, on the one hand, and the sensory source of the original experience of which this is a revival, on the other hand. For the after-image is proved to be an affair

[1] Quoted in James' *Principles*, II., p. 67.

[2] *Animal Magnetism*, p. 254.

[3] See *Animal Magnetism*, Binet and Féré, p. 253.

of the sense-organ — of the peripheral visual apparatus. If, then, the so-called after-image of the subjective memory image (imagination product) is really a physiological consequent of the subjective memory image (or rather of the physiological processes which lead up to it), as the regular visual after-image is the physiological consequent of the visual processes which lead up to the visual sensation, of which it is an after-image; then the subjective memory image would seem to involve retinal processes as part of the neural apparatus for its production. Anything which will prove or disprove the non-volitional character of this so-called after-image of the memory image, is then of decided importance in the question of the relation of sensational and revival processes.

It was for the purpose of meeting this situation that Miss Downey worked with her *naïve* subject. This subject had excellent power for visualizing color, could control the images well, and could project them. She was entirely ignorant of the complementary relations of colors of sensations and colors of after-images. She was also entirely in the dark as to the purpose of the experiments. In eighty per cent. (80 per cent.) of her trials the after color was the complementary of the induced color which it had succeeded. [1]

4. Miss Downey says that her subject did not believe she could change the course of an after-image voluntarily, and that she herself has never been able to do so. This is however improbable, as a consequence of her own experiment. We do not impugn her good faith in making the statement. But if one can create vivid color impressions and experience after-images of these, which bear the same relation to them that the ordinary visual after-image bears to the sense impression, it seems highly probable, as urged above, that the same retinal processes are involved in both the ordinary after-image and the subjectively aroused or memory process. If this is the case they ought to interfere with each other when it is attempted to carry them on simultaneously. It ought to be possible to interrupt and change the course of an after-image by voluntarily arousing subjective

[1] 'An Experience in Getting an After-image from a Mental Image,' PSYCH. REV., 8: 42.

colors. In other words it ought to be possible, if the above reasoning from the observed facts is correct, to *control* the course of the visual after-image, that is, to alter, voluntarily, its natural course.

Miss Washburn[1] found quite a considerable modification of the normal 'flight of colors' was produced by an effort persistently to see a given color through the course of an after-image. It was not possible to wholly submerge the regular colors of the after-image, but the color 'tried for' prevailed much more than when no effort was made. The subjects were practised until they obtained a fairly uniform course of color at each successive trial. They were then told to try hard to drown out the other colors by thinking persistently of a given color. With three of the subjects the traces of the color striven for were intensified and were held longer than in the normal after-image, and they also came sooner. With the other subject, who was an exceptionally good visualizer, the color striven for was held almost without interruption throughout the time of the after-image course.

II.

In view of the doubt that seems to prevail as to whether the course of the after-image can be altered or interfered with by mental imagery, it is worth while to look for further evidence on the question. And especially, in view of our ignorance as to the nature of the interference, if there is such, did it seem worth while to make further experiments along lines similar to some already reported. Our special desideratum was knowledge of the processes of the memory image, on the one hand, and of the after-image, on the other hand, that we might see the relations between them. In other words, we wanted to know how the 'ideational preparation' of the attention impinges upon and modifies the sensory process. In the case of the after-image we have a unique process. The peripheral apparatus keeps on sending in nervous disturbances, which arouse sensations, long after the external stimulus has been withdrawn, so that the possibilities of an even division of the attention between the memory image, which is called up voluntarily, and the peripherally

[1] 'Subjective Colors and the After-image,' *Mind*, N. S. (1899), 8 : 25–34.

aroused process, is very much easier than it is in the case where the peripherally aroused process is caused directly and immediately by an external stimulus. This is for the simple reason that we have to attend to the after-image in very much the same way that we attend to the memory image, whereas the sensation proper makes a much more forcible entrance into the focal region of consciousness. It is more compelling in its power over attention.

After the first preliminary work, which was directed to ascertaining whether there was a fair degree of constancy in the course of the after-image, we proceeded on the following plan: Each day we gave (1) a series of two to six normal after-images, usually with a twenty-second exposure, then (2) about two memory images, or subjective controls for a given color, beginning when no after-image effects were present, and then (3) two or more subjective controls, for a given color, of the after-image, this after-image being aroused in precisely the same way as the normal after-image. We found it necessary to have all these three kinds of experiment performed each day, in order to have a valid ground of comparison, for the different light conditions on different days led to very different color courses and lengths of courses in the after-image. This plan of procedure enables us to see, side by side (1) the simple after-image effect, (2) the simple memory image (imagination product) and then (3) these two combined.

The work was done in a dark room. The stimulus for the after-images was daylight admitted through a window, measuring 36 × 38 centimeters. This window was high up above the floor of the room so the subject, from his position, saw only the sky or clouds. A sliding shutter, with pulley attachments, made it possible to regulate the time of exposure very accurately. Some bars crossed the window, making it easier to follow the course of the after-image. The subject sat facing the window about six meters distant. The experimenter sat directly under the window, with his back to the subject. He worked by a small hand electric lamp, closely shielded in a box. The subject usually turned his head to another part of the room and closed his eyes, when observing an after-image. We do not

think our results were impaired by the small amount of light present, but of course absolute darkness would be preferable, as that would remove all doubt. Telephonic connections or an automatic recording apparatus would remove all necessity for light in the room where the observations were being made. An automatic recorder would be of further use in that it would make possible a more accurate transcription of the actual color changes. The subject himself finds it impossible to speak of all the changes, those he does speak of are already past when they are mentioned, and the experimenter may get them recorded for a still later time. Two reaction times enter into the error. Of course these hold alike of all records, and so their comparative value is not impaired. And they are used only comparatively here. An automatic recorder with receiving apparatus furnished with pressure taps for four or six colors, arranged in spectral order, would greatly facilitate work in this field.

The exposures were mostly of twenty seconds. In order to avoid fatigue, we usually changed off, experimenter becoming subject and vice versa, every fifteen minutes. And ample time was allowed in every case between experiments for the complete disappearance of the effects of the previous after-image.

Neither of us are exceptional visualizers. H. has always depended about equally upon the visual sense and the vocal muscular sense for the verification of spelling. When, a few years ago, as a beginner in psychology, his attention was first drawn to the work of Francis Galton on mental types, he found great difficulty in recalling the face of a near friend. The result of efforts at visualization were very disappointing. It seemed that objects of such worth must of necessity occupy a more prominent place in the visual imagination than that which this test revealed them to hold. As the visual attention was turned upon these thought objects, they seemed to vanish as do faces seen in clouds, or cliffs, or the moon, when they are closely scrutinized. But this power steadily improved, and H. can now visualize faces with ease, and can get a fair wealth of detail.

W. has always had a fair visual memory, being able to

remember dates, definitions, etc., best by having a visual image of a page or list of names. He had little difficulty in calling up the faces of friends. The visual image of a person was usually called up by the name of the person and was always connected with familiar circumstances. But these recalls were always very literal reproductions of the original experience. For example, he had seen Mr. Moody preaching and could easily see him in that position; but if he tried to see him in his study or at a hotel, it was no longer Mr. Moody. The image was very indefinite and unlike the original. When W. was about fifteen years of age, he often saw as he lay in bed, in the dark, 'flights of color.' These always had the same elliptical shape (about three inches by two) with an irregular, colorless spot occupying the center. This figure and these colors appeared about eighteen to twenty inches distant from his eyes. The phenomenon was of accidental origin, so far as W. is aware, and would appear and disappear of its own accord. But he was soon able to start it at will. When once set going, though, it went through its usual course. After a few months, this experience was lost sight of. It was recalled since he began working with after-images. But he cannot reproduce the figure, as he then had it.

This experience indicates a natural facility in W. for visual imagery. But since beginning this work, he has noticed a marked increase in this facility. His images are now more life-like, and he has no difficulty in calling up images of any familiar experience, such as a person or a landscape. Further he now has control of this imagery, so that he can see a familiar person in a situation very foreign to the person in question, such as the President of the university firing an engine or ascending in a baloon, things which he has never known him to do.

The charts presented herewith are reproductions of the courses of after-images, normal and controlled, and of controlled memory images. The principal object in view in presenting those on Chart I. is to show samples of the very early after-images. V., VI., VII., VIII., and IX. show a close similarity. These of February 24 show a marked increase in regularity over those of Feburary 10; though even these earlier ones are

by no means without similarity. The improvement is probably an improvement in the power of observing this species of phenomena. Chart II. shows, side by side, the normal after-image, the controlled after-image, and the controlled memory image or the voluntary revival and retention of color sensation, one series being by H. and one by W. In making up these charts as well as in making up the tables of averages it was necessary to adopt some arbitrary way of treating such colors as violet and orange, both for the purpose of simplifying the work of reproduction in the charts and in order to bring the summary statement of the results into a reasonable compass. The following compositions were adopted: Purple = two thirds red and one third blue, violet = two thirds blue and one third red, blue green = one half blue and one half green, orange = one half red and one half yellow, green yellow = one-half green and one half yellow.

The chief objection to this is that there is no means of distinguishing when these colors appear separately in different parts of the image and when they represent colors the same all over the field. But two simultaneous colors were comparatively infrequent. Spaces left without color indicate times when the image was colorless or had vanished from the field, temporarily or finally. The numbers at the left side of the charts represent seconds from the time of the beginning of the image.

Table I.

Name of Observer	W.	H.
Total Number of normal after-images observed	91	92
Average total time of the course of the normal after-image	171[1]	213
Average total time of *yellow* in the normal after-image	13	18
Average total time of *red* in the normal after-image	7.5	31.7
Average total time of *green* in the normal after-image	22	30
Average total time of *blue* in the normal after-image	42	75
Total number of controlled after-images	47	46
Average total time of the course of a controlled after-image	226	317
Number of after-images controlled for *blue*	19	23
Average total time of *yellow* in after-images controlled for *blue*	13	12
Average total time of *red* in after-images controlled for *blue*	6	28.6
Average total time of *green* in after-images controlled for *blue*	21	24.5
Average total time of *blue* in after-images controlled for *blue*	108	190
Number of after-images controlled for *green*	7	5

[1] Time is always given in seconds.

Table I.—*Continued.*

Name of Observer	W.	H.
Average total time of *yellow* in after-images controlled for *green*..	20	20
Average total time of *red* in after-images controlled for *green*......	16	27.6
Average total time of *green* in after-images controlled for *green*...	133	168
Average total time of *blue* in after-images controlled for *green*.....	21	66
Number of after-images controlled for *red*.............................	6	7
Average total time of *yellow* in after-images controlled for *red*.....	7.4	28
Average total time of *red* in after-images controlled for *red*..........	147	212
Average total time of *green* in after-images controlled for *red*.....	57	23
Average total time of *blue* in after-images controlled for *red*........	25	90
Number of after-images controlled for *yellow*..........................	15	11
Average total time of *yellow* in after-images controlled for *yellow*	45	102
Average total time of *red* in after-images controlled for *yellow*.....	10.6	22.6
Average total time of *green* in after-images controlled for *yellow*	36	18.5
Average total of *blue* in after-images controlled for *yellow*..........	30	53.6
Number of memory images controlled for *blue*........................	3	3
Average total time of *yellow* in memory images controlled for *blue*	14.5	16.6
Average total time of *red* in memory images controlled for blue...	5.6	24.6
Average total time of *green* in memory images controlled for *blue*	0	24.8
Average total time of *blue* in memory images controlled for *blue*..	123	228
Number of memory images controlled for *green*......................	4	4
Average total time of *yellow* in memory images controlled for *green*..	5.5	15.6
Average total time of *red* in memory images controlled for *green*.	16	20
Average total time of *green* in memory images controlled for *green*	62	134
Average total time of *blue* in memory images controlled for *green*	10.5	60
Number of memory images controlled for *red*........................	6	6
Average total time of *yellow* in memory images controlled for *red*	5.5	16
Average total time of *red* in memory images controlled for *red*...	99	172
Average total time of *green* in memory images controlled for *red*.	79	16
Average total time of *blue* in memory images controlled for *red*...	31	29
Number of memory images controlled for *yellow*....................	6	4
Average total time of *yellow* in memory images controlled for *yellow*..	93	161
Average total time of *red* in memory images controlled for *yellow*	27	23
Average total time of *green* in memory images controlled for *yellow*	18.6	38
Average total time of *blue* in memory images controlled for *yellow*	33	16.5

Table II.

	Blue.		Green.		Red.		Yellow.	
	W.	H.	W.	H.	W.	H.	W.	H.
Total time of color in normal after-image.	42	75	22	30	7.5	31.7	13	18
Total time of color in after-image controlled for *blue*.	**108**	**190**	21	24.5	6	28.6	13	12
Total time of color in memory image controlled for *blue*.	**123**	**228**	0	24.8	5.6	24.6	14.5	16.6

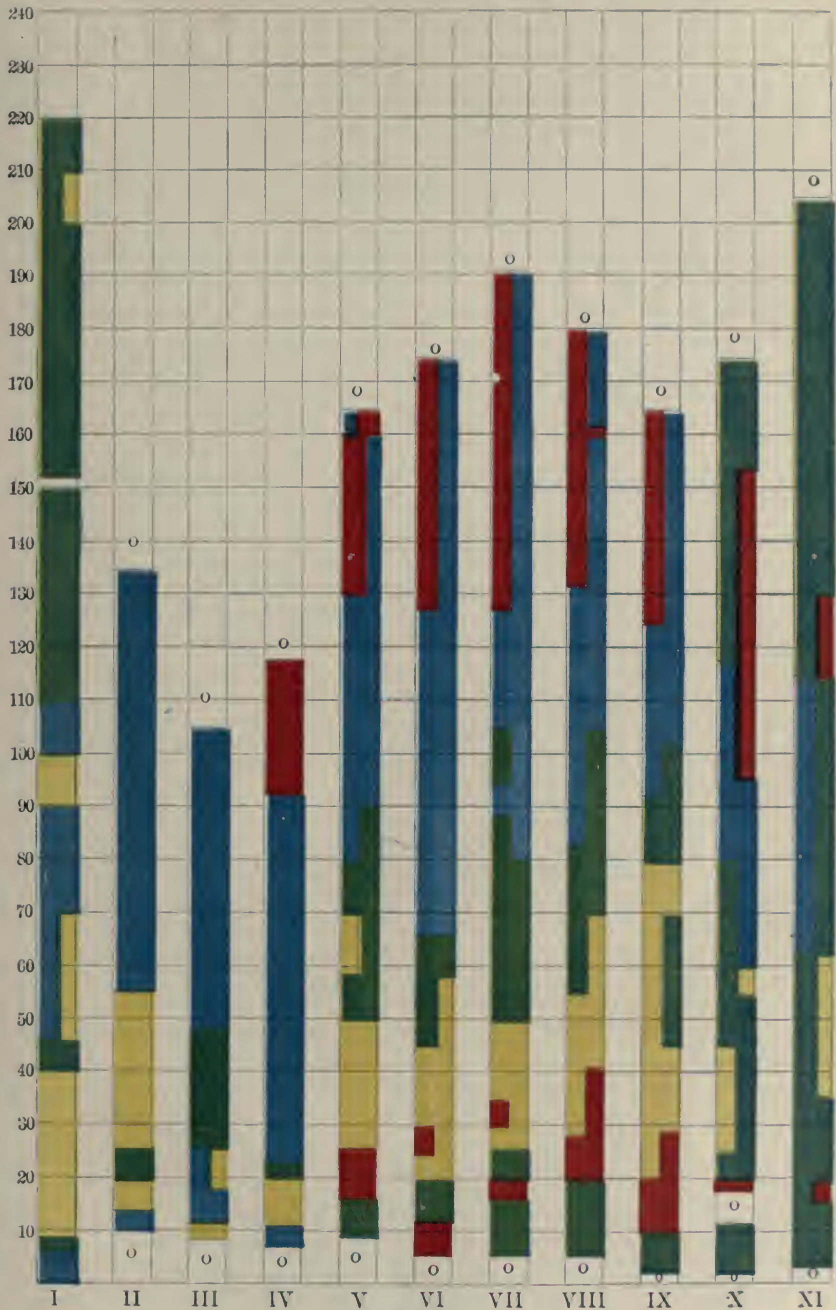
TABLE II. — *Continued.*

	Blue.		Green.		Red.		Yellow.	
	W.	H.	W.	H.	W.	H.	W.	H
Total time of color in after-image controlled for *green*.	21	66	**133**	**168**	16	27.6	20	20
Total time of color in memory image controlled for *green*.	10.5	60	**62**	**134**	16	20	5.5	15.6
Total time of color in after-image controlled for *red*.	25	90	57	23	**147**	**212**	7.4	28
Total time of color in memory image controlled for *red*.	31	29	79	16	**99**	**172**	5.5	16
Total time of color in after-image controlled for *yellow*.	30	53.6	36	18.5	10.6	22.6	**45**	**102**
Total time of color in memory image controlled for *yellow*.	33	16.5	18.6	38	27	23	**93**	**161**

A summary of the work of H. and W. is presented in Table I. This is the result of a very careful and complete analysis and averaging of 183 normal after-images, 93 controlled after-images, and 36 controlled memory images. One of the first things that strikes one as he looks at our scores of charts, or at this table, is the considerable increase of the total time of the controlled after-image over the normal. The latter increase the time about 70 per cent. in each subject. This longer time of the controlled image may be in part due to the engrossing of the attention in the work of distinguishing and holding the desired color, so that the time of the disappearance of the after-image is not noticed. In fact H. often reported, in the case of the normal after-image, that he no longer had an after-image,—that he now knew it was not present because he could move his eyes without affecting the image, but he did not know when it left. Its departure was not signalized by any observable change, but now that his arrested attention was turned to it he knew that it was not an after-image. If this was possible where the attention was directed wholly upon the after-image, it would be much more likely to happen when the attention was divided between the observation of the after-image and the revival of a color. W., however, never had this difficulty. He knew very clearly when the after-image proper was at an end. The lengthened time in his case then shows the effect of attention upon the after-image. It lengthens its life. We both feel

Explanation of Chart I.

I. W., February 10, normal after-image, 20 seconds exposure. II. W., February 10, normal after-image, 20 seconds exposure. III. W., February 10, normal after-image, 20 seconds exposure. IV. W., February 10, normal after-image, 20 seconds exposure. V. W., February 24, normal after-image, 20 seconds exposure. VI. W., February 24, normal after-image, 20 seconds exposure. VII. W., February 24, normal after-image, 20 seconds exposure. VIII. W., February 24, normal after-image, 20 seconds exposure. IX. W., February 24, normal after-image, 20 seconds exposure. X. W., February 24, memory image controlled for green. (No exposure.) XI. W., February 24, memory image controlled for green. (No exposure.)


240
230
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210
200
190
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170
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I
II
III
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VII
VIII
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X
XI
CHART I

Explanation of Chart II.

I. H., April 23, normal after-image, 20 seconds exposure. II. H., April 23, normal after-image, 20 seconds exposure. III. H., April 23, memory image controlled for blue. (No exposure.) IV. H., April 23, after-image controlled for blue, 20 seconds exposure. V. H., April 23, after-image controlled for blue, 20 seconds exposure. VI. W. April 28, normal after-image, 20 seconds exposure. VII. W., April 28, normal after-image, 20 seconds exposure. VIII,, W., April 28, memory image controlled for red. (No exposure.) IX. W., April 28, memory image controlled for red. (No exposure.) X. W., April 28-after-image controlled for red, 20 seconds exposure. XI. W., April 28, after, image controlled for red, 20 seconds exposure.

240
230
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VIII
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X
XI

CHART II

that this is the effect of attention directed to the after-image, independently of these objective results and this interpretation of them. Of course the factor that lengthens the after-image for H. by causing him to mistake a memory image for it, is ultimately the same as that attention factor which lengthens it for W. For the memory image which H. projects and mistakes for the after-image, because it is so vivid and his attention is not critical of the source of his experience, is created by this very activity of attention, which is one of the factors in the perception of the after-image. Having the attention more actively aroused, means the prolongation of the after-image, and with a little more arousal still, it seems possible to bring up a vivid substitute which may be mistaken for it. This relation of the after-image and the memory image must be taken up in the final section of the paper; but this much of the way seems to be pointed out in the pursuit of the question immediately raised by the lengthened course of the controlled after-image.

The point at which anyone shall conclude that his after-image has ceased and a memory image begun, is, it seems to us, a matter depending upon that person's experience in the comparison of these two kinds of mental contents. Probably both the subjective and objective factors are operative in all cases of mental activity of any sort. Those in which the subjective factor predominates are called subjective, and those in which the objective factor predominates are called objective. And on the border-land, as in these cases of the controlled after-image, it is a matter which will vary from individual to individual as to which he shall call objective and which subjective. Without some such special experience as afforded by these experiments, these two realms remain comparatively widely separated from each other; and it serves the purposes of life that they do. But such experience as we have had in this work tends to fill in the normal gap between them with a series of experiences, hardly different in themselves, and yet approaching both sides of the gap. In such conditions it is often difficult to know when the limit is reached. In fact in these experiences we find, as we expected to find, a very intimate interworking of the objective and subjective factors; and

this is one reason at least for the lengthening of the after-image when it is controlled.

This greater total time of the controlled image is the reason for the greater total time of a given color, in some cases, in the controlled image, even when another is controlled for, than in the normal after-image. As an extreme case, H. has yellow in the normal after-image only 18 seconds, while in the after-image controlled for red he has yellow for a total of 28 seconds (see Table II.). This increase is about proportional to the increase of the total time of the after-image when controlled. The same factor is operative, of course, in the increase of the time of the color controlled for; but the increase is so much greater in any and all of these cases that there can be no question but that there is another factor at work here. Take *e. g.*, the case of the increase of red in the image controlled for red whose amount of yellow was just now cited, and it is seen that the average amount of red has increased from 31.7 seconds in the normal after-image to 212 seconds in these controlled for red, in the case of H. So it is in the case of any color controlled for. The increase is so significant that there can be no doubt that the voluntary control is a very important element in *bringing in* the color in question.

In looking at the results as they are presented in Table II. (which is simply another form of some of the results shown in Table I., arranged with a view to comparisons of the sort made above) the question arises as to whether the color, complementary to the one controlled for, comes into any greater prominence than the other two. If red is the color controlled for, will green be more prevalent than in the cases where blue, for example, is controlled for? A glance at the table shows us that H. has less of the complementary of the color controlled for in every case, than he has of the same color in the normal after-image, while W. has relatively less of yellow in images controlled for blue and of blue in images controlled for yellow than in the normal after-images. There is an increase of red in images controlled for green and of green in images controlled for red. But there is also an increase of yellow in images controlled for green and of green in images controlled for yellow.

These increases are however not large enough to signify the influence of any special relation of green and yellow. It would seem, too, as though the above hint at a relation is not borne out in other parts of the table.

In comparing the average total times of a given color, in the after-image controlled for that color, and in the memory image controlled for that same color, we find a curious relation brought out. Green and red are each decidedly less in the memory image controlled for those colors respectively than in the after-images controlled for the same colors. In red we find the after-image at 147 and 212 seconds while in the memory image it is 99 and 172 seconds. Just the reverse holds in the cases of yellow and blue. Here the time of the color in the memory image is greater than in the after-image, and about as much greater as red and green are less. It seems more difficult to hold, subjectively, green and red than it is to hold, subjectively, yellow and blue. The interchange of these two colors between themselves, when one of them is being controlled for in the memory image, is more marked than it is with blue or yellow. The predominance of the complementary green in both controlled after-image and controlled memory images is well shown by the last four series on Chart II. (VIII., IX., X., and XI.), in all of which the control is for red.

Table III. presents results obtained from three other subjects, of the same general nature as the results from W. and H., but of a more preliminary nature. They seem worth presenting for the special reason that two of these subjects were especially poor visualizers at the start of their work, and yet present good evidence of a control over their after-images. One of these, M., described the memory image he had of blue as being 'like the fading blue after-image about a minute after the image had gone.' He as well as D., claimed never to have seen color in their memory images. Both of them are of the motor type. These subjects, though, both of them, show a marked increase in the average total time of every one of the four colors in an after-image controlled for the given color over the average total time of the same color in the normal after-image. This clearly indicates a power of subjective control in

TABLE III.

	Observer.	Average Total Time in Normal After-Image	Average Total Time in After-Image Controlled for Blue.	Average Total Time in After-Image Controlled for Green.	Average Total Time in After-Image Controlled for Red.	Average Total Time in After-Image Controlled for Yellow.
	D.	75.2	**90**			27.5
Blue.	M.	42.5	**70**	32.5	35	40
	Mc.	7.5		20	2.5	0
	D.	14.2	5			19.4
Green.	M.	17.8	25	**46.5**	10	20
	Mc.	7.5		**65**	5	0
	D.	35.6	12			10.1
Red.	M.	12.5	15	4	**20**	15
	Mc.	26		35	**92.5**	55
	D.	20.9	10			**50.6**
Yellow.	M.	16.7	0	10	25	**20**
	Mc.	11		0	12.5	**10**
	D.	4	1	0	0	4
Total No.	M.	6	1	2	1	1
	Mc.	4	0	1	2	1

cases where there is a minimum of the visual ideational factor in ordinary imagery.

III.

It seems very clear from these various lines of proof that there is an interference of the after-image and the memory image with each other. This also seems very clear to us from our experience with the work. W. felt that his effort to get an effective subjective control of an after-image led to bringing that control color up from *behind* the after-image. It started in a small area and spread out over the area of the after-image. Other colors tend to come in, presumably the colors proper to the normal after-image. There is a continual struggle, as long as the natural course of the after-image, between the color which one is trying to hold in the field and these other colors which are described as 'trying to assert themselves.' The two elements are competing for the same field. This effort to get and keep a given color in the field is a very distinct species of mental work. This is about equally true whether one is controlling an after-image or is trying to hold a given color against chance comers, — memory images. It is as distinctly work as if one should try to lift himself up out of a chair.

With H., the color, which he is trying to have control (suppress) the after-image, often comes as a halo around the after-image, and gradually closes in on it. It closes it out of existence. Other times the color desired comes as a spot of color of very indefinite outlines, off to one side of the after-image. This he may succeed in placing upon the after-image. Or he sometimes was able to change the visual attention from the one field to the other. In many cases the control color "blotted out" the after-image, form and color. Other times the control color came in on the form of the after-image. It is a clear case of competition for both of us.

But with the fact of interference established, the really interesting question first comes prominently into view. This, as already stated, is the question of explanation. What is the nature of this interference? This of course involves a knowledge of the nature of the processes themselves. And a very natural way of approach to this question is to inquire about the physical basis of these processes. This line of inquiry has been pursued very often. Brewster adopted it to explain spectral illusions, and such phenomena as that which Newton describes. He says it is 'a retransmission along the optic nerves to the retina' by which the retinal elements are again excited, and the image is had over again. And he could get a memory image of St. Paul's, of sufficient vividness to blot out the landscape which was before his wide open eyes, simply, as he thought, by this centrally aroused stimulation of the retina. Probably this explanation takes its rise in the feeling, which is very pronounced in such cases of strong and vivid memory images, that the image is a product of ones own willing. From this feeling that one is making it himself, it seems a very easy, and indeed natural, inference that the peripheral process is the result of some energy set free in the retina by an efferent stimulation of some sort.

There are efferent fibers in the optic nerve, known to be such, both from the relative position of axis cylinder and cell body, and from the particular connections in the central nervous system. It is quite possible that these fibers function in such a way as to stimulate the peripheral sensory neurons. And if so we have herein the mechanism of the revival process. The

mechanism, whatever it is, not only serves for the production of spectral illusions, pseudo-hallucinations, and the more vivid species of memory images. For there is only one species of revival. The causes of revival are indeed many. But the mechanism of the process itself must be one and the same. And if this is the *modus operandi* of revivals of this more vivid sort, it is also the way of revivals of every sort. It is highly probable that all revival involves the functioning of the peripheral sensory apparatus, to some extent. It will at once be said that this is certainly not an essential condition of revival. For the blind by accident, who lose their retinæ entirely, no more lose the power of visual revival than does the patient who has lost a leg by amputation lose the power of reviving the muscle and joint sensations which he formerly had from that member. To this we simply say that the cases of the blind who have lost their whole peripheral sensory visual apparatus are worthy of very careful psychological investigation. The chances are that some important discoveries with regard to the nature of the revival process await us here. And further that there is undoubtedly a vicariousness in the nervous system by which processes are pared down. Part processes are left out. Nature is always making short-cuts. In this way it may be that the *tendency* to discharge toward the peripheral apparatus, there to set up the revival of the sensory process, is sufficient in itself to engender in the central (cortical) sensory cells a process which stands for the revival of the sensory process. It is a *symbol* of a copy.

From introspection we find that this *symbol* of revived sensory experience is further removed in kind from the genuine revival of a sense image, than is the latter from the sense-image itself. This is what we should expect if our theory of their origin is correct. W. calls them memory images and visual images, respectively. The former (symbols of copies) do not have any color. They are the images most of us have when we are engaged in conversation, and do not have time to get good visual or other sensory images of all the objects mentioned. They are the images which our subjects D. and M. have at their best, when they are trying hardest to get visual

images. They cannot get what W. calls visual images. The after-image, the vivid sensory image, and the ordinary memory image constitute in fact, a series of psychic processes which grade into each other. They differ among themselves in the nature of the interplay of the peripheral and central factors, and the relative importance of each, as suggested above.

The difference between sensational content and revived content, is not merely difference in intensity, as Hume said. It is probably, in part at least, a difference in content itself, as suggested already. It is also well known that memory of an experience is not the mere revival of that experience. If it were merely the experience over again it would not be *recognized* as that former experience. Psychologically considered, the difference between the sensational and revived processes is one of the *organization* of the processes within themselves, rather than of the impetus of the nerve current along any particular channel. Our whole view of the nature of psychical processes has very much changed since Hume. The concept of vividness is a very important aid to the explanation of such a difference as this now under consideration.

Vividness is indeed another name for the organization obtaining in experience. We commonly mean by it that emphasis which a mental process or an experience gets by the direction of attention toward that part of the field of consciousness. The common illustration of the watch ticking and being unnoticed as long as we are occupied with other things, but coming into prominence so soon as we turn our attention to it, is a good one. The loudness of the tick is the same all of the time. But the inner factor changes. That which was 'fringe' now becomes focal in consciousness. More of the energy of attention is directed toward it, and so it occupies a relatively more important place in the field.

The difference between a sensational experience and a memory of it is largely a matter of the changed vividness of many of the part processes involved. As Dr. Sidis[1] expresses it, in characterizing the difference between perception and hallucination, there is a difference in the *nucleus* in the two experiences.

[1] PSYCHOLOGICAL REVIEW, XI., pp. 15–29 and 104–137.

The changed vividness of elements changes the mental center of gravity, placing the emphasis on a new part of the experience. Parts which were focal before, are now in the fringe, and the present focal elements were previously fringe elements. This, at least, is one important species of change which occurs as the sensation becomes memory. Of course there is also the possibility of an actual change of the content, as contemplated above. Some elements may wholly drop out, and other new fringe elements may be brought in. It seems that recognition of the reproduced mental content is largely dependent upon such fringe elements.

The criticism of Hume's psychology inheres in part in all attempts to deal with this problem after the manner of a structural psychology. For this matter of the changing vividness of the various parts of the given organic experience is not in itself the whole of the changes that occur. There is something in the organization as such that escapes us when we analyze the experience. And this something has a very real part in giving its character to the revived process as distinct from the original experience. This, the structuralist says is mere form and so counts for nought in the final estimate of the make-up of the processes. This is an untenable position. We must heed these hints as to significance, which the functional view can give us. This help is especially valuable in this present case. The very essence of the difference between the sensory and the memory process consists in the different meaning attaching to them individually. But, using the functional view as it should be used, as a guide for the analysis of the structural method, the present study serves as an example of the possibility of explaining these significances, in part at least, by the aid of such analyses. The difference between a given sensational experience and the memory of the same experience, which is clearly one of meaning, is largely accounted for in our analysis as content differences. It lies chiefly in the different *start*, or *awakening*, of the peripheral sensory processes and in the different relative parts played by the various factors.[1]

[1] The MS. of this article was received July 14, 1904.—ED.

FROM THE UNIVERSITY OF CALIFORNIA PSYCHOLOGICAL LABORATORY.

COMMUNICATED BY G. M. STRATTON.

VII. The Effect of Verbal Suggestion upon the Estimation of Linear Magnitudes.

BY JOSEPH E. BRAND.†

Late Assistant in the California Psychological Laboratory.

The purpose of this investigation was to find how far, and in what direction, the visual estimation of a linear magnitude could be affected by suggestion of certain possible errors in such estimation, the subject knowing that the suggestions were purely arbitrary, *i. e.*, that they had no reference to any foreseen tendency to err in either direction.

The general method of the investigation was to show the subject for a brief time two small objects at a predetermined distance apart, and to let him subsequently place two similar objects so as to include between them an interval which he judged to be equal to that between the first two. The suggestions were introduced by showing to the subject along with the standard interval certain printed mottoes containing the random suggestions of possible errors in the reproduction of the interval; *e. g.*, 'Don't make too long,' 'Make Short,' etc. Then from the results of his reproductions the attempt was made to determine whether or not the suggestion had produced any effect.

Three different forms of apparatus were employed, but as the differences were chiefly mechanical, and the principle of operation identical in all, the description of the second form will suffice.

Two low, dead-black screens were built up on a table. The screen nearer the subject was high enough so that when seated his horizontal line of sight passed just over it. In the farther screen there were just above this line two long horizontal slots,

†Deceased.

the lower one being 2.2 cm. in width, and the upper one 1 cm. in width, having behind them backgrounds of the same blackness as the screens. Through the lower slot and against this background were exposed the printed slips bearing the mottoes, resting on a wooden ledge fastened to the back of the screen just below the edge of the slot. On a similar ledge behind the upper slot were exposed two white pegs .8 cm. in diameter and long enough to have their ends hidden by the screen. These pegs could be moved along the ledge, and hence adjusted for various intervals between them. On the near side of the nearer screen and just below the top, was a ledge on which the subject moved little pegs similar to those just described, and thus reproduced his estimation of the standard interval.

Over the front of the farther screen was made to slide a movable screen having a single horizontal slot through which either of the two slots in the stationary screen could be exposed, but not both together. Thus the subject saw either the motto in the lower slot, or else the standard distance marked off by the two white pegs appearing as rectangles 1 × .8 cm. in the upper slot, or else both slots were altogether hidden. The distance from the subject's eye to the nearer screen was 40 cm., and from the nearer screen to the farther was 80 cm.

The mode of operating the apparatus was sufficiently simple. The proper motto being in the lower slot, and the pegs having been properly spaced in the upper, and both slots being hidden by the movable screen, the operator, after due warning to the subject, raised the screen one notch and exposed the motto for two seconds; then raised it a second notch and exposed the standard interval for two seconds; then dropped it one notch and left the motto again exposed. The subject immediately so placed his pegs on the ledge as to mark off his estimate of the space interval, and the operator recorded the length so marked, reading it from a scale hidden from the subject's vision by a narrow strip of black paper projecting above the ledge.

1. First Set of Experiments.

In this first work only two subjects were engaged and a variety of suggestion mottoes were used, viz., '*Make short enough,*

'*Make long enough*,' '*Don't make too long*,' '*Don't make too short*,' '*Make short*,' '*Make long*,' a nonsense motto '*Zwp fjvic bgzx asye*,' and a meaningless sentence '*Life is real where*.' These mottoes were printed in black capitals 1.2 cm. high on white cardboard.

In groups *A*, *B* and *C* four standard lengths for estimation were used, 16, 22, 28 and 34 cm. In group *D* standards of 24, 26, 30 and 32 cm. for subject C. and 12, 14, 18 and 20 cm. for subject Y. Long standards were used for subject C. in this group because from examination of results in the preceding groups it seemed that the long standards would give more definite results; and since standards for this subject were changed, it was thought advisable to change standards for the other subject also. Thus any possible difficulty from too long use of the same standards was obviated. The four standards employed in any group were given in succession with each motto, in an order determined by lot, the order of succession of the mottoes being changed for each day. A 'nonsense motto' was used in order that the tests without suggestion might be under conditions as like as possible to the others, except for the suggestion itself.

Tables I. and II. give in detail the results of this investigation. Instead of finding the average lengths reproduced with different mottoes it was deemed simpler to interpret the data by aggregation. The number tabulated under a given motto for a given day is therefore the sum of all the judgments taken that day for that motto. As remarked above, the series in both tables are arranged in four groups, which differ amongst themselves in regard to the mottoes and standards used, and also in that a month's interval elapsed between the work of group *C* and that of group *D*. For convenience in examining the tables, totals are given under each group for such columns as it is desirable to compare. As all the mottoes are not used on all the days, only such days are included in forming the totals as make the totals in the same horizontal line properly comparable.

An examination of these tables leads to some interesting conclusions. First, we find by comparison of daily totals and general totals, that the suggestion produces a definite, though slight, effect. The results for 'Make short enough' are in both

Table I.

Based on 650 Judgments; Subject, Miss C.

	1	2	3	4	5	6	7	8	9	10	11
	Day of Group.	No. of Judgments Each Motto.	Standard (Sum).	Nonsense Motto.	"Life is Real Where."	"Make Short Enough."	"Don't Make Too Long."	"Make Long Enough."	"Don't Make Too Short."	"Make Short."	"Make Long."
Group *A*	1	9	189	199		—	212.7	—	—		
(Eight different	2	8	167	179		—	—	180.7	—		
standards, 11–30	3	10	202	221.5		227	225	223.5	224		
cm. long, were	4	10	202	218		224.5	214	220	223		
used in this	5	10	202	—		220	221.5	216	221		
group).	6	15	320	343.6		—	341.5	—	342.9		
Sum of days 3, 4, 5		30	606	—	—	671.5	660.5	659.5	668		
3, 4, 5, 6...		45	926	—	—	—	1,002	—	1,010.9		
3, 4, 6...		35	724	783.1	—	—	780.5	—	789.9		
	1	11	248							259.2	258.3
Group *B*	2	16	322							302.5	305.2
(Standards, 16,	3	24	588							584	597.6
22, 28, 34 cm.)...	4	24	660							650.7	680.3
Sum......		75	1,818							1,796.4	1,841.4
Group *C*	1	8	200	216.6	205		210.2		211.7		
(Standards, 16,	2	9	210	196.2	202.1		196.3		205.4		
22, 28, 34 cm.)...	3	9	204	200.5	198.2		194.3		202.1		
Sum......		26	614	613.3	605.3		600.8		619.2		
	1	6	170	—	—		183.2		175.3	172	185.5
Group *D*	2	10	276	279.1	279.1		284.6		280.5	279	279.6
(Standards, 24,	3	7	206	209	211.6		221.7		213.6	203.3	212.5
26, 30, 32 cm.)...	4	6	162	165.6	157.3		165.2		156.2	164.5	170
S. of days 1, 2, 3, 4		29	814	—	—		854.7		825.6	818.8	847.6
2, 3, 4...		23	644	653.7	648		671.5		650.3	646.8	662.1
Total, *C* & *D* (except day 1)......		49	1,258	1,267	1,253.3	—	1,272.3	—	1,269.5		
Total, *A* (except days 1, 2) *C* & *D*		100	2,354	—	—	—	2,457.5	—	2,455.7		
Total, *B* & *D*......		104	2,632	—	—	—	—	—	—	2,615.2	2,689

cases greater than those for 'Make long enough'; the results for 'Make long' are greater than those for 'Make short' for subject C., and also greater for subject Y. in group *D*, although less in group *B* for this subject. Similarly, results for 'Don't make too long' are pretty uniformly greater than those for 'Don't make too short' for subject Y., and also for subject C. in group *D*, but not for this observer in groups *A* and *C*.

TABLE II.

BASED ON 598 JUDGMENTS; SUBJECT, MR. Y.

	1	2	3	4	5	6	7	8	9	10	11
	Day of Group.	No. of Judgments Each Motto.	Standard (Sum).	Nonsense Motto.	"Life is Real Where."	"Make Short Enough."	"Don't Make Too Long."	"Make Long Enough."	"Don't Make Too Short."	"Make Short."	"Make Long."
Group *A* (Eight standards 11–30 cm. long).	1	9	189	176		—	176.5	179.7	—		
	2	10	202	188		197	188	192.5	190.5		
	3	10	202	186.5		191	190	194.5	186.5		
	4	15	320	297.6		289	293.8	—	289.2		
Sum of days, 2, 3, 4		35	724	672.1		—	671.8	—	666.2		
2, 3...		20	404	374.5		388	378	387	377		
Group *B* (Standards 16, 22, 28, 34)...............	1	21	534							503.3	487.8
	2	22	538							499.9	491
	3	19	514							499.8	500.9
	4	20	524							487.2	488.5
Sum...........		82	2,110							1,990.2	1,968.2
Group *C* (Standards 16, 22, 28, 34)...............	1	11	320	283.9	276.8		285.1		287.8		
	2	11	278	252.5	248.8		258.5		255.6		
	3	9	198	176.5	178.1		175.1		172.2		
Sum...........		31	796	712.9	703.7		718.7		715.6		
Group *D* (Standards 12, 14, 18, 20)...............	1	6	98	94.7	91.7		92		94.3	92.2	92.8
	2	6	90	86.6	86.3		89.8		88.1	85.6	92.4
	3	6	102	94.9	94.3		94.3		91	91.4	95
Sum...........		18	290	276.2	272.3		276.1		273.4	269.2	280.2
Total, *C* and *D*.....		49	1,086	989.1	976		994.8		989	—	—
Total, *A* (except day 1) *C*, *D*......		84	1,810	—	—		1,666.6		1,655.2	—	—
Total, *B* and *D*.....		100	2,400	—	—	—	—	—		2,259.4	2,248.4

Since group *B* followed group *A* and group *C* followed group *B* without gap, but a month elapsed between groups *C* and *D*, and since the difference in the presumable effects of the same mottoes occurs only between group *D* and the other groups, we must conclude that the uniformity is too great to admit of an explanation except by potency of the suggestion from the mottoes.

Second, this suggestion-effect varies both according to the individual to whom the suggestion is made, and also according to circumstances. The disagreements just referred to as existing between group *D* and the preceding groups is evidence upon the

latter of these points. The data within these groups are reasonably self-consistent, showing that on almost every separate day the same effect was produced, but that during the month's interval the subject had gotten over into a condition such that the difference between the effects produced upon him by two opposing formal suggestions was of opposite sign to what it was earlier.

Third, the mere words 'long' and 'short,' regardless of their content, seem to affect the estimation under certain circumstances. This is illustrated by Table III., which gives the sums for the mottoes containing the word 'short' and the sums for those containing the word 'long' from group *D* of both tables.

TABLE III.

Subject.	No. of Judgments.	Standard.	Mottoes Containing the Word	
			"Long."	"Short."
C.	58	1628	1702.3	1644.4
Y.	36	580	556.3	542.6

From this table it will be seen that the total for mottoes containing the word 'short' is less in both cases than that for mottoes containing the word 'long.' Similar totals from group *A* give exactly opposite results for Table I., and neutral results for Table II. (*i. e.*, in the latter case the totals are about equal).

Now these results, taken as they are merely from the group totals, may either be the result of chance, or they may be due to the existence of a different attitude towards positive and negative suggestions, causing the subject at any given time to incline to act in accordance with one and in opposition to the other; or they may be due to a tendency to be influenced by the mere words 'short' and 'long,' as said above. The likelihood of this latter explanation led to the group of experiments which follow under section 2.

Fourth, a motto which has interest for a subject seems to give greater lengths in the reproduction than an uninteresting one. The nonsense mottoes in group *A* (except for a single day for subject Y.) give smaller totals than do the other mottoes, which in this part of the experiment were not yet so familiar as

to lack interest; while in groups *C* and *D* the motto 'Life is real where,' which the subjects declared was much more empty and uninteresting than the 'nonsense' motto, and hence should be taken as the criterion in these groups, gives smaller totals than the averages for the suggestion mottoes, as show in Table IV.

TABLE IV.

Subject.	Mottoes with content. (Average of columns 7 and 9 of groups *C* *D*, omitting row 1 of *D*, Table I.)	Mottoes without content. (Total of column 5 of same days.)
C. (Table I.)	1270.8	1253.3
Y. (Table II.)	991.9	976.0

The indication of this comparison is of course very unsatisfactory, but seems at least to warrant a special investigation on this point.

2. SECOND SET OF EXPERIMENTS.

The apparent effect of the mere words 'long' and 'short' in the first set of experiments led to the second set, in which the mottoes used were only three in number, viz.: 'long,' 'short,' and 'XXXX.' The apparatus differed from that described above only in the substitution, for the pegs, of white paper squares on a black screen, one square of the pair being on a strip of black paper running in grooves behind a slot in the screen, so that the adjustments, *i. e.*, the various distances of separation of the squares, were obtained by simply sliding the strip along. Three standard distances were used, viz.: 17, 18 and 19 cm., being given in such order that each was preceded by each of the others about an equal number of times, and each of the three used an equal number of times on the same day. Each of the mottoes was given an equal number of times with each of the standards, in order determined by lot. The letters of the mottoes were so spaced as to cover the same extent in every case and so exclude the possibility of a difference due to mere space contrast or assimilation.

Four subjects were employed, and the results were not very uniform, two of the subjects showing no decided tendency towards anything resembling a constant effect, while the other

two subjects, showed a clear general constancy of considerable difference throughout. The results for these two are given in Tables V. and VI.

TABLE V.

SUBJECT K.

Date.	Standard.	Times Used.	Sum for Standard.	Sum for "Long."	Sum for "Short."	Sum for "XXXX."
Sept. 18.	17	3	51	47.8	48.4	48.3
	18	3	54	53.6	52.4	53.9
	19	3	57	54.8	56.2	58.0
			162	156.2	157.0	160.2
Sept. 24.	17	5	85.	79.9	79.2	81.5
	18	5	90	84.4	84.3	85.1
	19	5	95	93.5	95.5	94.3
			270	257.8	259.0	260.9
Sept. 25.	17	6	102	94.6	98.6	98.3
	18	6	108	104.9	104.4	103.0
	19	6	114	107.5	113.6	111.7
			324	307.0	316.6	313.0
Sept. 26.	17	6	102	92.7	96.8	95.1
	18	6	108	106.6	107.2	105.0
	19	6	114	110.4	112.1	112.9
			324	309.7	316.1	313.0
Oct. 2.	17	6	102	94.1	95.5	92.7
	18	6	108	101.8	104.5	103.8
	19	6	114	111.4	114.8	113.7
			324	307.3	314.8	310.2
Oct. 4.	17	4	68	58.1	62.3	60.6
	18	4	72	67.2	65.7	68.6
	19	4	76	72.7	73.4	73.2
			216	198.0	201.4	202.4
Grand Total.		90	1620	1536.0	1564.9	1559.7

By examining Tables V. and VI. it will be seen that for Subject K. the figures for the motto 'short' are with two exceptions greater than the figures for the motto 'long,' and for subject M. the reverse is true, again with two exceptions. In the daily totals, however, there are no exceptions. In spite of the fact that the other two subjects gave neutral results, the hypothesis that the words 'long' and 'short' of themselves are capable of influencing the estimation of distances seems well

TABLE VI.

SUBJECT M.

Date.	Standard.	Times Used.	Sum for Standard.	Sum for "Long"	Sum for "Short."	Sum for "XXXX."
Sept. 5.	18	6	108	108.3	103.2	108.8
	19	6	114	123.2	117.0	116.8
			222	231.5	220.2	225.6
Sept. 6.	17	7	119	118.2	113.0	118.5
	18	5	90	92.8	91.3	86.1
			209	211.0	204.3	204.6
Sept. 11.	18	8	144	144.6	140.5	141.7
	19	6	114	116.0	117.8	112.1
			258	260.6	258.3	253.8
Sept. 13.	17	8	136	139.8	134.6	137.8
	18	8	144	148.3	145.5	146.6
			280	288.1	280.1	284.4
Oct. 7.	17	7	119	117.3	116.0	119.0
	19	7	133	130.2	134.3	134.7
			252	247.5	250.3	253.7
Oct. 11.	17	7	119	121.0	115.0	117.4
	19	7	133	136.7	132.5	138.0
			252	257.7	247.5	2554
Oct. 14.	17	1	17	17.1	16.0	17.0
	18	3	54	50.3	48.9	52.9
	19	4	76	76.2	72.7	75.1
			147	143.6	137.6	145.0
Grand Total.		90	1620	1640.0	1598.3	1622.5

grounded, for in view of the discussion of the first group of experiments we might expect that the suggestion effect would be contrary in certain different subjects, and lacking in others. Thus the influence of purely formal and arbitrary suggestion seems even more clearly evidenced by this second group of experiments than by the first.

VIII. Experiments on the Unreflective Ideas of Men and Women.

BY GENEVIEVE SAVAGE MANCHESTER.

In 1891, Professor Jastrow made a study[1] of the mental differences of men and women, using as material, lists of one hundred words each, written by men and women students in his classes. These lists were written as rapidly as possible in order that they should be natural and unreflective. From a comparison of twenty-five men's lists, with an equal number of the women's lists, he concluded that the feminine traits of mind revealed by the study are: 'An attention to immediate surroundings, to the finished product, to the ornamental, the individual, and the concrete, while the masculine preference is for the more remote, the constructive, the useful, the general, and the abstract.' A few years later, a similar experiment was made at Wellesley College. Miss Nevers,[2] who made the study, found that her results were strikingly different from those obtained by Dr. Jastrow. Later, however, it was discovered that for the most part, this difference in results was due to a deviation in method, the instruction to write the lists as rapidly as possible, having been omitted by Miss Nevers. A repetition of the experiment conforming closely to Professor Jastrow's procedure produced results which supported some of his conclusions, but not all.[3]

[1] *New Review*, Vol., V, 1891, pp. 559 to 569.

[2] PSYCHOLOGICAL REVIEW, 1895, pp. 361 to 367.

[3] Other experiments on the mental differences of men and women have been carried on, though not along the lines suggested by Professor Jastrow. Helen Bradford Thompson, in a study of the mental differences of men and women came to the following conclusions (*Psychological Norms in Men and Women*, Univ. Chic. Press, 1903, page 171): "Women are decidedly superior to men in memory, and possibly more rapid in associative thinking. Men are probably superior in ingenuity. In general information and intellectual interests there is no difference characteristic of sex." For other references and results v. Havelock Ellis' *Man and Woman* (Contemp. Science Series).

Preliminary to a further study of the mental differences of the sexes, I have repeated Dr. Jastrow's experiment at the University of California. To get the required lists, all the men and women in several classes in general psychology were given blank sheets of paper on which were spaces for 100 words, the writer's name, sex and the time required to write the list. The only instructions given were to write at top speed and to avoid writing words in sentences. From the large number of papers received, three sets were selected, each set containing twenty-five men's lists and twenty-five women's. In selecting the lists, the only requirements were that the lists should seem natural and unreflective and that the same word should not appear more than once in the same list; that is, that each list should furnish 100 different words. It is possible that this last requirement may have been a deviation from Professor Jastrow's method. Upon inquiring, he wrote me that he was not certain whether the Wisconsin lists had been kept free from repetitions or not. With a very few exceptions, the same word does not appear twice in any one paper in the California lists. Having selected the lists, the words of each set were then separately tabulated under the following twenty-five heads, the words written by the men and women being kept apart in each set: (1) animal kingdom, (2) verbs, (3) proper names, (4) adjectives, (5) implements and utensils, (6) abstract terms, (7) wearing apparel and fabrics, (8) vegetable kingdom, (9) building and building materials, (10) parts of the body, (11) geographical and landscape features, (12) other parts of speech, (13) miscellaneous, (14) interior furnishings, (15) meteorological and astronomical, (16) mineral kingdom, (17) occupations and callings, (18) conveyances, (19) stationery, (20) foods, (21) educational, (22) arts, (23) amusements, (24) mercantile terms, (25) kinship.

After classifying each of the three sets separately, they were then combined and the set thus obtained consisting of seventy-five men's lists and seventy-five women's lists, was classified as the smaller sets had been.

The division of the words under the above twenty-five heads follows the classification of Dr. Jastrow in order that the Cali-

fornia results might be comparable with those of Wisconsin, although certain objections to the division might be urged. For example it might be pointed out that the procedure of Dr. Jastrow does not admit of exact repetition, though this is essential to a correct testing of results. No two experiments would tabulate the words in the case of the twenty-five classes under exactly the same heads. This difficulty arises from the fact that some of the classes are vague. An illustration of this lack of clear definition may be drawn from the class 'interior furnishings.' Many of the household articles that women use, are of course, 'implements and utensils' and just what household articles should be classed as 'interior furnishings' and what as 'implements and utensils' is not clear. In case most of the implements women use about their work are classed as 'interior furnishings,' the preponderance of the men in the implement group loses all its significance. The class 'foods' is not clearly to be distinguished from the 'animal,' 'vegetable' and 'mineral kingdom' groups, since all foods can be classed under these three heads. Here again there is a chance for words to stray. The class 'educational' is also exceedingly vague. Each person who uses this system of classification will probably classify under this group somewhat differently. The personal factor will come in to an appreciable extent in each repetition of the experiment and absolute uniformity of method will not be secured.

These defects are easier to see than to remedy. If a classification free from them is possible, it must probably be made along more strictly logical lines. The words would need to be classified several times instead of once, selecting in each classification some one principle of division.

In two cases, I have made slight changes in Dr. Jastrow's terminology. For his term 'unique words,' *unrepeated words* has been substituted, and for the term 'different words,' *vocabulary* has been used.

In Table A the results of the California experiments are given, together with those previously reported, arranged in the order in which the experiments were performed.

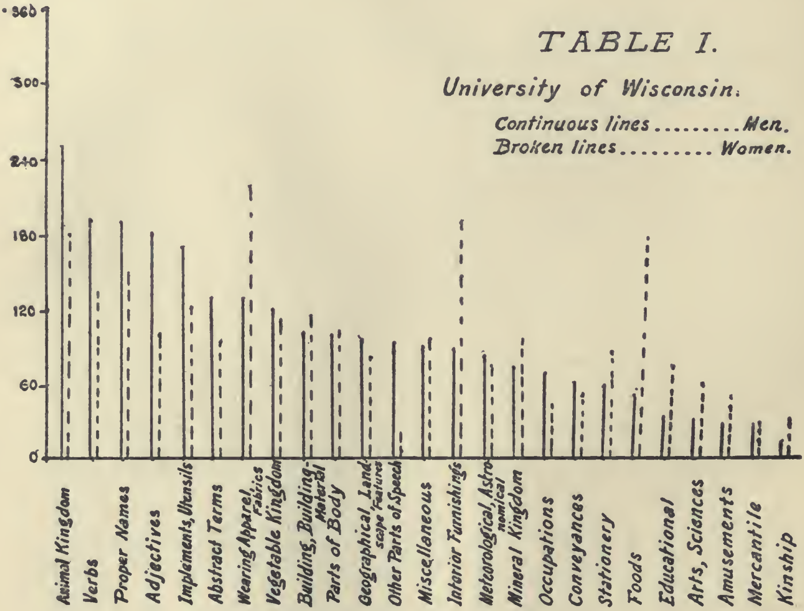
To aid in comparing the results, they have been represented

TABLE A.

	Wisconsin.		California Set I.		California Set II.		California Set III.		Welles-ley.	Combined California Set.	
	Men.	Women.	Men.	Women.	Men.	Women.	Men.	Women.	Women.	Men.	Women.
1. Animal kingdom	254	178	193	196	239	196	210	170	146	642	562
2. Abstract terms	131	97	137	117	152	114	51	71	101	340	302
3. Adjectives	177	102	207	267	201	247	216	283	300	624	797
4. Amusements	30	53	25	39	27	51	23	45	17	75	135
5. Arts	33	61	47	79	54	84	75	75	17	176	238
6. Buildings, building materials	105	117	127	131	116	145	120	144	86	363	420
7. Conveyances	62	52	40	43	42	48	51	58	19	133	149
8. Educational	34	76	68	69	59	97	50	55	102	177	221
9. Foods	53	179	104	109	85	81	53	45	88	242	235
10. Geographical, landscape features	97	80	113	104	110	116	83	123	70	306	343
11. Implements, utensils	169	121	86	45	114	99	144	103	139	344	247
12. Interior furnishings	89	190	64	82	78	128	129	147	212	271	357
13. Kinship	17	32	10	9	4	11	14	16	42	28	36
14. Mercantile terms	30	29	10	11	7	15	22	17	18	39	43
15. Meteorological, astronomical	85	76	64	53	102	99	91	110	109	257	262
16. Mineral kingdom	74	96	88	33	109	69	90	71	30	287	173
17. Miscellaneous	91	97	257	255	147	134	188	151	123	592	540
18. Occupations	71	47	47	30	58	31	76	44	24	181	105
19. Other parts of speech	96	5	112	146	90	73	123	90	164	325	309
20. Parts of the body	101	105	92	60	122	73	58	54	66	272	187
21. Proper names	194	153	98	106	99	128	56	41	81	253	275
22. Stationery	60	86	38	35	57	47	78	81	69	173	163
23. Vegetable kingdom	121	110	97	102	99	78	53	90	101	249	270
24. Verbs	197	134	286	257	259	218	360	299	279	905	774
25. Wearing apparel, fabrics	129	224	89	119	70	118	86	117	97	245	354
Totals	2500	2500	2499	2497	2500	2500	2500	2500	2500	7499	7497

graphically by a system of vertical lines, the lengths of which are proportionate to the numbers given in Table I. The continuous lines represent the number of words in each class used by the men, the broken lines, the number of words written by the women.

The order of arrangement of the classes in the diagram is, of course, arbitrary. For convenience, the classes are arranged in the order of their size, as obtained in the results of Professor Jastrow's men. The series begins with the class 'animal king-

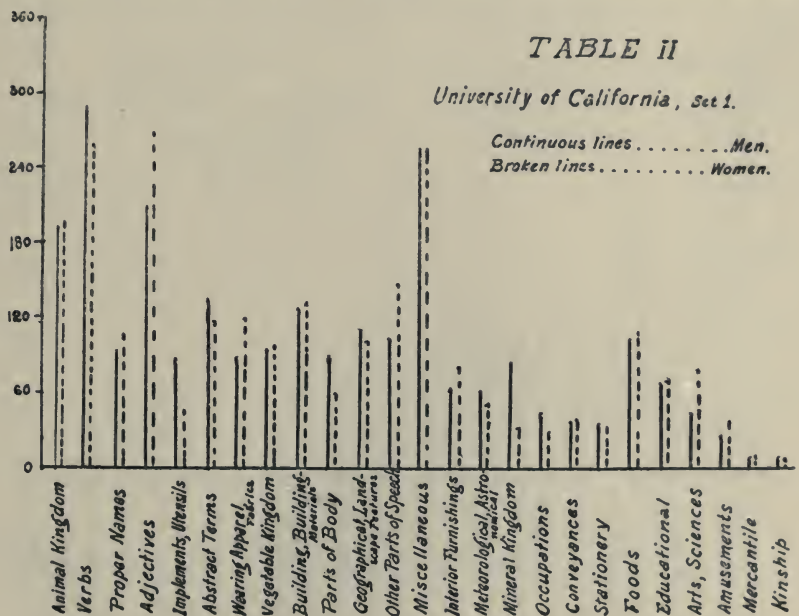


dom,' to which the men of the University of Wisconsin contributed the largest number of words, and ends with 'kinship,' the class to which they gave the smallest number of words. This order is kept for the women of the Wisconsin lists and for all the subsequent lists.

Turning now to Table I. to review the comparison of the Wisconsin men and women, it is seen at a glance that the women greatly preponderate over the men in the class of 'wearing apparel and fabrics,' 'interior furnishings' and 'foods'; to a less degree in the classes 'educational,' 'arts' and 'amusements.' In

mentioning terms denoting objects in the 'animal kingdom,' 'adjectives,' 'abstract terms' and 'implements and utensils,' the men exceed the women. Numerous other variations are shown, but these are the most striking.

A comparison of these results with those obtained by the California experiments discloses the fact that, while there is general agreement in several interesting particulars, there is nothing that can be called complete corroboration of the conclusions Professor Jastrow drew.[1] A prominent characteristic to be



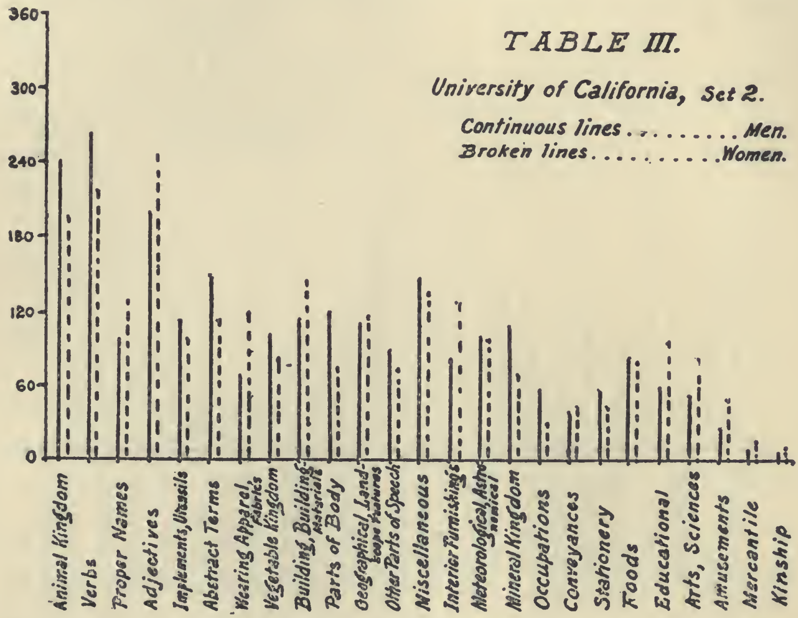
noted in the California experiments is the absence of the marked disparity between the men and women which the Wisconsin record shows. In the class 'wearing apparel and fabrics,' the California women clearly exceed the California men but in a less degree than the Wisconsin women exceed the Wisconsin men. (Compare Tables II., III., IV. and V. with Table I.)

The same relation holds for the class 'interior furnishings'; but in the case of 'foods,' one of the three classes in which the

[1] The fact that there are no men's lists in the Wellesley material is an obvious drawback in comparing them with Wisconsin and California lists and for that reason they will, for the time, be left out of account.

Wisconsin women markedly exceeded the Wisconsin men, the result differs from that obtained by Professor Jastrow. In one of the California sets, the women exceed the men slightly (see Table II.), while in the other two sets, the men are slightly in advance of the women.

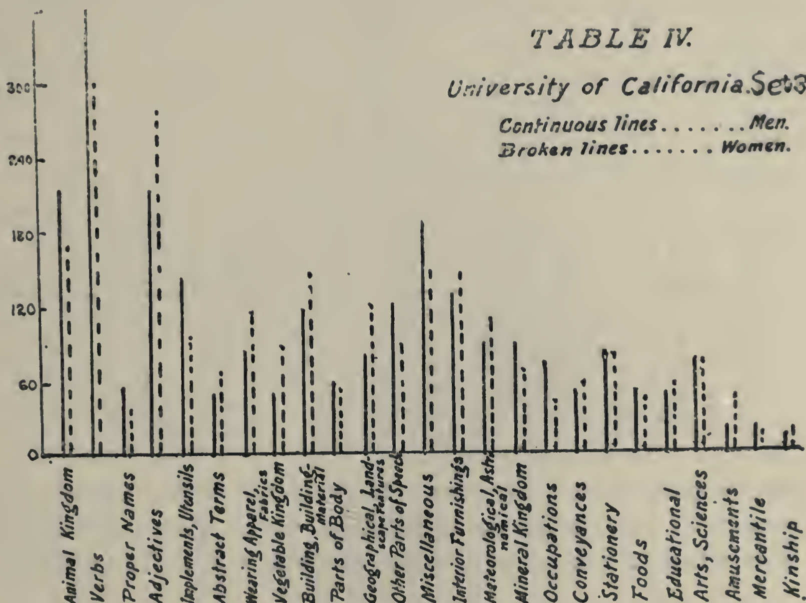
The California lists agree with the Wisconsin lists in that the women exceed the men in the classes 'educational,' 'arts,' and 'amusements'; but as before, the California men and women differ less from each other than the Wisconsin men and women



The notable ratios in which the Wisconsin men exceeded their women classmates in terms belonging to the classes 'animal kingdom,' 'proper names' and 'adjectives' do not hold for the California lists. The California sets all agree with the Wisconsin lists in that the men lead the women in every case in mention of 'verbs' and 'implements and utensils.' A general agreement of both Wisconsin and California results is to be found in the fact that both men and women drew the larger part of their words from the classes arranged on the left half of the tables, while the classes to the right, such as 'educational,'

'amusements,' 'mercantile terms' and especially 'kinship' furnish comparatively few of the surface ideas.

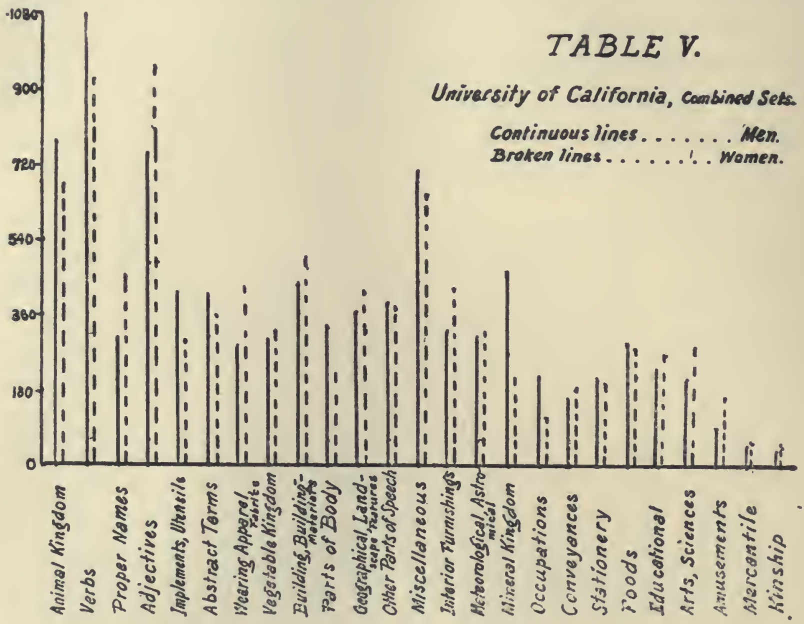
In the Wisconsin lists, Dr. Jastrow found that the vocabulary of the men was greater than that of the women. The same result was obtained in all three of the California sets. In Table VI. the results of this part of the experiment are tabulated, the results of the Wisconsin and Wellesley experiments being included.[1]



A study of this table shows that in each of the five sets, the men have used a larger percentage of different words than the women of the same set. In set II. of the California series, however, the women exceed the men of set I. in vocabulary. It is to be remarked that the women in all three of the California sets and also in the Wellesley set, exceed the Wisconsin men in vocabulary. It is also to be noted that the difference between the vocabularies of the Wisconsin men and women is considerably greater than that between the California men and women.

[1] The column headed 'percentages' indicates the ratio of the vocabulary to the total number of words written.

The vocabulary of the Wisconsin men is 10 per cent. larger than that of their women classmates, while, in no case, does the vocabulary of the California men exceed that of the California



women more than 4 per cent. In every set, both the men and women in the California experiment use larger vocabularies than either the men or women of Wisconsin. It would, of

TABLE VI.

	Unrepeated Words.		Vocabulary.		Percentages.	
	Men.	Women.	Men.	Women.	Men.	Women.
Wisconsin.	746	520	1376	1123	55	44.9
California Set I.	1000	949	1471	1397	58	55.9
California Set II.	1170	1079	1583	1509	63.32	60.36
California Set. III.	1079	978	1489	1407	59.56	56.28
Combined Calif. Set.	1975	1950	3119	3048	41.58	40.64
Wellesley		868		1306		52.25

course, be unwarranted to conclude from these results (though the thought is at least suggested) that the men and women of California have more diversified interests that those of Wiscon-

sin, and that the men and women of California differ from each other less than the men and women of Wisconsin. Only one set of lists is given here for the University of Wisconsin, and additional sets might change the proportions materially. The matter of the treatment of plurals may account in great part for the larger vocabularies obtained in the California experiments. I have in all cases counted the singular and plural of the same idea as two different words. The justification for this is to be found along psychological rather than etymological lines. Etymologically, the words *horse* and *horses* are practically the same, but the mental picture, or idea, corresponding to each is different. If Professor Jastrow did not make this distinction, but counted the singular and plural forms of an idea as the same word, the vocabularies in the Wisconsin study would be correspondingly low.

That the men exceed the women in vocabulary in each of the five sets, is significant, even if the superiority on the part of the men is slight. It should be remembered that in the combined California set, the total number of words was three times as large as in the other sets. It is to be noted, further, that not only the percentage of difference, but the absolute difference between the vocabularies of the men and women is greater in the 2,500-words sets than in the 7,500-word set. In the smaller sets, the difference varies from two per cent, to a little more than three per cent. while in the large set, the difference is leveled to one per cent. It seems likely that if the number of lists were indefinitely increased, the difference in vocabulary between the men and women would diminish regularly as the limits of the language were approached.

Leaving now any further comparison of the California results with those obtained by other experiments, a more profitable field for study lies in the examination of the cumulative results of the experiments at Wisconsin and California, in order to discover in what features, if any, all of the results agree. If this study is to disclose any real differences between the unreflective ideas of the sexes, such differences should be evident in all of the sets of lists, or at least in a pronounced majority of them. The most significant differences, then, revealed by these

experiments, will be those which appear in every one of the four sets.

To indicate the agreement or disagreement of the different sets as regards the classes of words used Table VII. was prepared.

TABLE VII.

+ MEN LEAD. — WOMEN LEAD.

	Wis.	Calif. I.	Calif. II.	Calif. III.	Calif. Comb.
Animal kingdom	+	—	+	+	+
Verbs.	+	+	+	+	+
Proper names.	+	—	—	+	—
Adjectives.	+	—	—	—	—
Implements and utensils	+	+	+	+	+
Abstract terms	+	+	+	—	+
Wearing apparel, fabrics	—	—	—	—	—
Vegetable kingdom	+	—	+	—	—
Buildings, building materials	—	—	—	—	·
Parts of body	—	+	+	+	+
Geographical, landscape	+	+	—	—	—
Other parts of speech	+	—	+	+	+
Miscellaneous	—	+	+	+	+
Interior furnishings	—	—	—	—	—
Meteorological, astronomical	+	+	+	—	—
Mineral kingdom	—	+	+	+	+
Occupations	+	+	+	+	+
Conveyances.	+	—	—	—	—
Stationery.	—	+	+	—	+
Foods.	—	—	+	+	+
Educational.	—	—	—	—	—
Arts, sciences	—	—	—	—	—
Amusements.	—	—	—	—	—
Mercantile terms.	+	—	—	+	—
Kinship.	—	+	—	—	—

In this table, the classes in which the men excelled the women in the number of words written are marked by a plus sign, while the classes in which the women led are indicated by a minus sign.

A review of the results of the classification into the twenty-five groups, shows a rather remarkable agreement in the five sets tabulated in Table VII. Of the twenty-five classes, eighteen show an agreement, either in all five cases or in four cases out of five. In the three classes in which the men lead most pronouncedly, namely, in 'verbs,' 'implements and utensils' and 'occupations,' the notion of action is prominent. In the classes in which the women lead, such as 'adjectives,' 'wearing apparel and fabrics,' 'interior furnishings' and 'buildings

and parts of buildings,' no such notion is evident. On the contrary, these show a preference for things at rest.

Running through all, or nearly all, of these agreements in results, there appears, beside the notion of action as contrasted with inaction, another element. In the cases in which the men lead, time is an essential factor; in the cases in which the women lead, space is the more prominent consideration. While the time and space conceptions are closely allied to the notions of action and inaction, they seem, nevertheless, to be not altogether identical. A further inference may be drawn from the cases in which there is agreement in all five sets. The fact that the men throughout draw a large percentage of their words from such classes as 'implements and utensils,' 'occupations,' and 'verbs' than the women do, and from the fact that the women lead throughout in the classes 'wearing apparel and fabrics,' 'buildings and building materials,' and 'interior furnishings,' it may be inferred that the unreflective ideas of both men and women concern the objects with which they are familiar and in which they have considerable interest. That the classes 'implements and utensils' and 'occupations' are of special masculine interest, will probably not be questioned, but such an interpretation of the verb class needs justification. This justification is found in the character of the verbs used. In a very large percentage of the cases in which men use verbs, these verbs are suggestive of action in the field of men's especial interest.

To make this clear, two lists of verbs are submitted, one secured from three men's papers and the other from three women's, the lists being taken at random in both cases. Such words as 'address' and 'telegraph,' classed here as verbs, may also be nouns. In such cases the context was taken as a guide in determining the sense in which the writer used the word. The lists follow:

A. (1) address, (2) telegraph, (3) fall, (4) rise, (5) call, (6) forget, (7) ride, (8) play, (9) ringing, (10) reading, (11) studying, (12) work, (13) play, (14) tick.

B. (1) shoot, (2) tick, (3) twitch, (4) cure, (5) hit, (6) miss, (7) aim, (8) blow, (9) shoot, (10) sail, (11) hoist, (12) lift, (13) pump, (14) rush, (15) study, (16) judge.

The first series of verbs was taken from women's lists, the second from men's. The masculine character of the second series is unmistakable and was so judged by several persons to whom the lists were read without disclosing the actual source of each group.

With this further knowledge of the verbs used, it seems safe to say that the evidence justifies the statement that the unreflective ideas of men are controlled by the familiar and interesting acts and objects of their lives. Similiarly, the classes 'wearing apparel and fabrics,' and 'interior furnishings,' are recognized categories of peculiar feminine interest. It might be suggested, however, that buildings and building materials are not distinctly familiar and interesting to women. The objection falls when the character of the words drawn from this class is known. The words used were names of particular buildings and parts of houses, such as court house, church, gate, door, floor, fence, steps, marble. There is scant mention by the women of such distinctive building materials as bricks, mortar, cement and stone.

Just why women should exceed men in the classes 'arts,' and 'educational' is not evident, but taking the cue from the former cases, it seems that it might be because these classes of objects are more familiar to women than to men and of relatively more importance to them. Though men are the chief creators of art, women are more familiar with the ordinary art products, such as pictures and musical compositions. Similarly in educational matters, the women were probably more impressed with the parts of the educational system, such as lectures, texts and examinations, owing, perhaps to the fact that many of them were preparing to become teachers.

That the women lead in the category of amusements, is probably due to the fact that women, as a class, have more leisure than men. With the time to enjoy amusements of various kinds, women indulge more in them. Hence these things are more familiar to women than to men and come more readily to mind when there is a call for a rapid gathering of ideas.

Leaving now the cases where there is an agreement throughout all five sets, the agreements which appear in four

sets out of five deserve a word. In the classes 'animal kingdom,' 'abstract terms,' 'parts of the body,' 'miscellaneous,' 'mineral kingdom' and 'other parts of speech,' the men exceed the women in four sets out of five. The mention of objects belonging to the 'animal kingdom' is plentiful in the lists of both men and women, the difference between the number of times such words are written by men and women being, on the whole, not very large, but the fact is notable that in all cases, but one, the sex naturally most familiar with the various members of the 'animal kingdom' is the one which leads in this class. In the class 'abstract terms,' a similar condition prevails. While the total numerical lead of the men over the women is not great, it is, nevertheless, to be found in four cases out of five. That men have more interest and training in mathematical, physical and philosophical abstractions, might account for this difference. The significance of the men's lead in mention of 'other parts of speech' is not clear. In the classification used, this term embraces conjunctions, prepositions, interjections, pronouns and adverbs. Since the class was not subdivided, it is impossible to tell in which of the four parts of speech the men markedly excel. As they lead in verbs, it might be expected that they would lead in adverbs. So too, it might be expected that men would lead in prepositions and conjunctions since these deal with abstract relations.

Men also lead the women four times out of five in mention of 'parts of the body.' Just what, if anything, this implies, I have not determined, though it may be that the greater importance of the parts of a man's body, as tools for his daily use, keeps the conceptions of them more constantly in his mind.

That men lead in the class 'miscellaneous' may mean any one of several things. If the other groups in the classification happened to be more adapted to catch the surface ideas of women than those of men, the 'miscellaneous' class would be correspondingly large for the men. Another explanation of the men's superiority here might lie in the fact that men are interested in a greater number of objects and activities.

The lead of the men in the 'mineral kingdom' is probably due to the fact that they carve and chisel, mine and build, while women ordinarily do not.

'Conveyances' might have been discussed under the head of 'amusements,' as most of the words in this class seemed to be of that general character.

That the women in all of the California sets lead in the mention of adjectives seems significant. It has been found in the classes discussed before that the surface ideas of men concern action, the tools used in the performing acts, and the differentiations of particular acts into occupations; that is, their ideas are related to construction. The women, on the other hand, excel the men in the mention of articles of dress, house fittings, parts of houses, particular buildings and art products. There seems to be a real principle of difference here in the general character of the words written by the men and those written by the women. Men speak of the process of creating, women of the thing created. This is the same conclusion reached by Dr. Jastrow.

In view, then, of this tendency of the feminine mind toward things, rather than the doing of things, it seems natural that women should be more familiar with the qualities of things than men are. This preponderance of adjectives in the women's lists seems to bear out this supposition.

In the mention of terms of kinship, it is of interest to note that both the men and the women draw very sparingly from this class. This fact seems rather in opposition to the hypothesis before advanced that the surface ideas are of things familiar and interesting, for of course one's kindred are usually the persons seen oftenest and considered of greatest importance. It is probable that unconscious or even conscious selection played a part here. It is very likely that the persons writing the lists considered this work a sort of official act since the papers were to be returned to an instructor, and they would naturally refrain to a certain extent, from speaking of parents and other relatives. The very attitude the mind took in performing the task would inhibit ideas drawn from the family life.

In the classes which have not yet been discussed, 'proper names,' 'meteorological and astronomical terms,' 'stationery,' 'foods' and 'mercantile terms,' there is not enough regularity to warrant the drawing of any conclusions. In the class 'proper names,' it would be expected that the women would lead con-

siderably, since they show a preference for the concrete, rather than the abstract, but in two sets out of five, the men are ahead. Again, the natural expectation would be that men would write many more astronomical and meteorological terms than women, but in this class the men lead in only three cases out of five. Oddly enough, the men on the whole, in the California sets, lead slightly in foods, and the women in mercantile terms, very different results from those obtained by Dr. Jastrow.

Coming now to the last feature to be examined, namely, the time required to write the lists, it is seen from Table VIII. that the average time for the women of California is 5 minutes and 39 seconds. The average time for the men of California is 5 minutes and 47 seconds.

TABLE VIII.

	Average for Men.		Average for Women.		Average for Men and Women.	
Wisconsin					5 min.	8 sec.
California I.	6 min.	2 sec.	6 min.		6 min.	1 sec.
California II.	6 min.	17 sec.	5 min.	55 sec.	6 min.	6 sec.
California III.	5 min.	2½ sec.	5 min.	2 sec.	5 min.	2 sec.
Wellesley			5 min.			
Combined Calif.	5 min.	47 sec.	5 min.	39 sec.		

As Professor Jastrow does not give the average time for each sex separately, and as there are no men in the Wellesley list, these sets are not available for this comparison.

In each of the California sets, the time the women required to write the lists is slightly less than that of the men. This may mean, either that the women associate more quickly, or that they write more rapidly. That each student kept his own time renders these results less trustworthy than they would otherwise have been.

Having now presented the evidence at hand, it only remains to review the differences in the unreflective ideas of the sexes as suggested by these experiments.

From the results of the classification into the twenty-five groups, the following general statements in regard to the surface ideas of men and women may be made:

1. The surface ideas of both men and women pertain to objects which are familiar and interesting.

2. The dynamic aspect of objects is more attractive to men, while the static or completed aspect appeals more to women.

3. Time as a factor enters more largely into the surface ideas of men; space is more often a prominent feature of the surface ideas of women.

4. Men make a greater use of abstract terms, while women show a preference for the concrete and for descriptive words.

From the tabulation of the words used into unrepeated words and vocabulary, it is found that the range of the surface ideas of men, as a group, is slightly greater than that of women.

From the examination into the average time required for writing the lists, it is found that women are able to write one hundred associated surface ideas in somewhat less time than men.

All of the specific tendencies above mentioned seem to give concurrence to a general principle of difference between the sexes. The surface ideas of men are extensive rather than intensive, while the opposite is true of women. This conclusion is supported, not only by the fact that men show a preference for abstract terms, for action and for the time idea, while women prefer the concrete, the completed object and space relations, but also by the facts that men have a greater range of surface ideas than women, and to a certain extent by the fact that women have their reflective ideas more at hand, as shown by their shorter list-time. Men are interested in far-reaching relations existing between things; women give more attention to the minute analysis of things themselves.[1]

[1] The MSS. of these Studies were received October 17, 1904. — ED.

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THE PSYCHOLOGICAL REVIEW.

THE PRESENT STATE OF PSYCHOLOGY AND ITS RELATIONS TO THE NEIGHBORING SCIENCES.[1]

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I.

To-day we have arrived at the conviction, that though the great and complex totality, which we call reality, cannot be understood without more or less artificial isolation of elements and without an analytical investigation of the mutual relations of such elements, yet the elements, which our science so distinguishes, are not to be considered as the constituent elements of the reality itself. In other words: the conditions of knowledge and of existence are not the same. Our ways of understanding are not necessarily the ways nature follows in her production. This is the old fundamental thought of critical philosophy, which has slowly made its way, especially during the later years, not only among philosophers, but also among naturalists who have discussed the first principles of their science. Then the possibility appears of an irrational relation between thought and reality, — the possibility, that the analysis of thought cannot do justice to the great synthesis of reality. The validity of science does not suffer by this, because the analyses and distinctions, which we undertake in order to arrive at a scientific understanding, ought to be founded, point by point, in observations of the living and concrete reality.

[1] Address delivered at the Congress of Arts and Sciences, St. Louis, September, 1904.

In no domain of experience does this point stand out so clearly as in psychology. In the material world, the elements which we distinguish have their position in space, one outside another, though an interaction is supposed to exist between them. But such an external relation cannot be valid in the domain of mental life. Here, the single element is so woven into the whole that its very character is determined by it, and the whole is here not to be considered as a mere product of the elements. In psychology, analysis and distinction have a more artificial character than in physical science. We have less right still to consider mental elements as absolute realities, than to look at material atoms in this way. However, there is no other way to scientific understanding of mental life than analysis on the basis of observation and experiment. And at its first beginnings, as at its limits, mental life has a sporadic character, presents itself, at least apparently, as isolated sensations, so showing a great contrast to the character of totality and synthesis, which it has where it appears in full development and maturity. We have here an antinomy which is of great importance to all psychological research. We cannot explain mental life as a mere product of the elements distinguished by the analysis, neither as a product of the sporadic flashes, to which it seems to be reduced at its limits; and yet the elements, to which the analysis and the observation of limits conduct us, cannot but bear an inward relation to the concrete consciousness and its synthetic totality.

This antinomy has had a great influence on the development of psychology. It manifests itself especially in the struggle between the two great schools, the one founded by Hartley and Hume and continued in the association-psychology and the Herbartian school, the other founded by Leibniz and Kant and continued in the idealistical school of Germany. The first school leads to a mental atomism, while the other maintains the synthetic character of mental life. I am not going to follow up the history of this struggle in its particulars. There is no psychologist, whose general standpoint and special views are not determined by his position as regards the relation between atomism and synthesis in the domain of mental life. There is a

temptation to dogmatize on both sides. They may each consider its particular point of view as an absolute and all embracing one.

There is a psychological atomism which looks at the elements of psychological analysis as absolute and real parts, mechanical composition of which produces the mental life. It forgets that the whole psychological problem begins anew in every mental atom. For as in physical science the atoms, which seemed to be absolute, turned out to be worlds apart, in the interior of which movements take place and currents go on, so we are led to acknowledge that our simplest sensations are synthetic phenomena, concrete totalities, corresponding to more or less complex physiological processes.

On the other hand, there is a dogmatism, which looks at mental life as an undivisible unity, perhaps a substantial unity, which defies all analysis. It forgets that mental life, as indeed all life, exists under a perpetual struggle against internal and external oppositions, and that even practical introspection discovers important points of difference; for instance, at every choosing between possibilities. Different dispositions and tendencies manifest themselves in consciousness. The points of difference, then, are not called forth by scientific observation and analysis, but they belong to life itself. As scientific analysis by its one-sidedness always forces us to return to the great synthesis of life, so on the other hand, we are forced by the internal tension of life to acknowledge the reality of differences and oppositions and so far to verify the results of scientific analysis. Only what life has connected, can be analyzed by us; but this connection does not exclude differences between the elements of life.

In American literature the relation between analysis and synthesis in psychology has been energetically investigated by such eminent thinkers as William James and Hugo Münsterberg. I believe that the last-mentioned thinker has been led to put up a stronger contrast between psychology and life than it is possible to maintain. It is always life which gives to psychology its materials, and introspection does not begin in scientific analysis, but is a practical necessity which presupposes the existence of points of real difference within the totality of life.

The contrast between analysis and synthesis in the domain of psychology has to a certain point an affinity with the contrast between intellectualism and voluntarism. In the domain of sensations and ideas the distinction between elements can be made with the least difficulty. It is the most articulated side of mental life, and at the same time the side which is most open to observation and experiment. The life of emotion and will shows a greater concentration, and the synthetical character here shows itself more clearly. The over-valueing of the results of the analytical method very naturally lead one to undervalue the importance of emotional and volitional life, and even perhaps to look at emotions and will as mere resultants of sensations and ideas. But it is impossible to deduce the mental concentration from the interaction of absolute elements, and the whole direction of the development of sensations and ideas is determined by the interests, values and aims, which have their foundation and find their expression in emotional and volitional life. The voluntarism, which was founded by Fichte and Schopenhauer and has important support in the biological theory of the struggle for life, is more and more considered as the main point of view in psychology.

II.

Both the incommensurability between analysis and synthesis, and the superiority of voluntarism compared with intellectualism ought to diminish the propensity to close once for all the conception of personality, as theology and speculative philosophy have often tried to do. Positivism and empirical philosophy are often accused of abnegating the conception of personality, and in our time the historical view and the theory of liberty are often contrasted with empirical psychology. But even the empirical, experimental and analytical school of psychology presupposes an energetic and earnest recognition of the reality of personal life. This school is founded on the conviction, that the value of mental life is not to be diminished by being bound to certain conditions and subjected to certain laws. It studies then, with confidence, mental life in all ways which are open to science.

The difference between the psychological schools depends on where the problem is found, and how the burden of proof is distributed. Is the riddle of psychology how unity and continuity in mind are possible, or does the riddle arise, when consciousness appears in a sporadic manner, in isolated flashes? That is the main question. But it branches out into many particular questions. The task of the synthetic school is to find the special forms of unity and continuity, which cannot be deduced *a priori*, and then to explain, how it is possible, that mental life in certain cases can have a sporadic character. The task of the other school is to describe the particular forms and degrees of isolation, and then to explain, how there can be unity and continuity in mental life. Every school of psychology ought to admit, that so long as mental life persists, a perpetual struggle is going on between the synthetic and the sporadic tendencies. When the character of unity prevails, the problem is, whether this unity is a mechanical aggregate, or whether it has a deeper foundation.

Pathological psychology seems to me decidedly to prove the truth of the synthetic conception. Without continual mental labor the 'psychological tension' (to use the expression of M. Pierre Janet) cannot be sustained; and in mental disease this tension, without which consciousness cannot unite within itself a varied content of different elements, can only be maintained with great and painful effort; very strong influences are then necessary, if division or slackness are not to be the results. Isolated and sporadic phenomena are always setting mental energy a task.

The conception of mental energy can, as all conceptions of energy, only be defined by the labor which is performed, the resistance which is conquered. There is so much more mental labor to be performed, the more elements or tendencies there are that have to be united in the same mental state; the more different these elements or tendencies are, the stronger each of them is, the more intimately they are to be united, and the more remote in time they are from one another and from the present moment. It is true, that in the individual cases it will be a matter of no little difficulty to apply that concept of psy-

chical energy, whose possibility here appears. The true factors of psychical energy can only be determined by careful observation and all-sided knowledge of the special historical and individual conditions in each single case. The number of elements or tendencies, the degree of their difference, the intensity of each one of them, the intimacy of their connection, the degrees of their distance in time, all this it is difficult to point out with certainty, and all five vary from case to case. To all this is yet to be added the velocity with which the mental function of synthesis is to be performed. We do not here have such simple factors as mass and velocity, by which physical energy is determined.

An inexhaustible wealth of possibilities is conditioned by the very different ways and degrees in which these five separate circumstances may appear. There is here a great field for observation, experiment and comparison. The comparative psychology of individuality is as yet in an elementary state. Only in the domain of psychology of religion, especially here in America, a movement has begun in this direction. But no theory can ever give an exhaustive description of the manner in which the different elements or tendencies work together in any single state of a single individual. Here, as everywhere, the perfect individualization is to be attained by art, not by science. Art only can give a synthesis, which in some measure can do justice to the great synthesis of life.

III.

New problems arise when we try to characterize the relation of psychology to the neighbouring sciences. Psychology has a special relation, on one side to physical, and on the other side to historical and ethical science. And the relation can be briefly said to be, that in comparison with physical science psychology has a decidedly synthetic character, but in comparison with historical and ethical science a decidedly analytical character. By these contrasts the problems which arise at the limits of the different sciences are determined.

I have already mentioned that the simplest mental elements which we can distinguish correspond to very complex physio-

logical processes. What psychologically appears quite simple is a physiological multiplicity. In a simple mental element must be combined what physiologically covers several moments and a whole region of the brain. But there is also another thing which is of importance here. Mental elements are qualitatively different one from another, while we have reason to believe that the correspondent processes in the brain are only different as regards intensity, direction and combination. What psychologically appears as differences of quality is from the point of view of physical science to be regarded as differences of quantity. Continuity, then, is more easily demonstrated from the physical than from the psychological point of view. The old maxim that nature does not move in bounds cannot be carried out in psychology as entirely as in physical science.

From these circumstances some thinkers have concluded that a science treating of mental life is only possible, if for the relation between mental states we can substitute the relation between the corresponding states of the brain. In order to be a science, psychology must be transformed into physiology. If not, it should, according to these philosophers, be impossible to approach the ideal of scientific understanding, *i. e.*, the pointing out of continuity and equivalence between phenomena. But we always begin by discovering causal relations between qualitatively different phenomena, and not till later on can we take up the task of substituting for this elementary causality a more perfect causal relation with continuity and equivalence between the phenomena. Though in the domain of psychology we are scarcely able to go further than to the elementary causality, because we have no mental units and so no thorough quantitative methods, yet this fact does not exclude the right to admit a causal relation between mental states. And this is not only a right, but also a necessity. If there exists a causal relation between the correspondent processes of the brain, there must also be at least an indirect causal relation between the mental states. Moreover, we have only quite schematical constructions of the corresponding processes of the brain, constructions which are based on analogy with the directly observed and analyzed mental states. From these states we draw our conclusions as to the

logical processes in the brain. This conclusion can not be true, if psychological observations and analysis are not correct. The independence of psychology is thus presupposed.

Perhaps the simplicity and the qualitative character of the mental elements are to be regarded as the results of a hidden synthesis, so that if we could penetrate more deeply into the sphere of mental differentials, for instance, to differentials of the second or the third order, the whole problem would stand in a clearer light to us. But we should always here meet at last the great problem of the relation of mind and matter. Here, also, the contrast between analysis and synthesis becomes important. The difference which can be established between mind and matter is due to analysis, to a distinction of elements, which in reality exist in connection with one another. We break the real totality, and afterwards we are astonished, because it is difficult to unite the parts into which we have divided it. This point has been very well cleared up by Wilhelm Wundt and Roberto Ardigo. The reality is always the great fundamental synthesis, within which we move with all our abstractions and analyses. It is a full unbroken melody, compared with our laborious spelling. But there is no other way to knowledge than the one which begins with analyzing. Our attention proceeds from point to point, and only later on tries to unite its single results. And as little as we ought to ascribe absolute validity to our distinctions, so little ought we to regard it as fortuitous, that our seeking after knowledge necessitates just these special distinctions. It is one of the characteristics of reality, that it can only be comprehended by careful analysis of its contents.

I am not here going to discuss the hypotheses of the relation of mind and brain. I shall only say that as the physiology of the processes of the brain do not depend on other methods or points of view than those of physical science in general, the duty of proving is incumbent upon him who maintains an encroachment of the mind on the physiological processes. Such a supposition would do away with the independence of physiology. But there is no reason to deviate from the principle which physical science has followed for centuries, and to which all its tri-

umphs are due, namely, that material phenomena are to be explained by material causes. Even to-day the dictum of Spinoza is valid: 'When men say that this or that action of the body springs from the mind, they do not know what they say, and they do nothing but confess that they know nothing about the cause of the action.' The only working hypothesis which makes possible a coöperation between physiology and psychology without any encroachment from either side, regards the relation of mind and matter as a functional relation, in the mathematical sense of the word, and tries to find as much continuity within both series of phenomena as possible. A final metaphysical interpretation is still an open question, but psychology as such has nothing to do with it. The parallelism, or, as I prefer to call it, the hypothesis of identity, has mostly been assailed as a metaphysical hypothesis. But it is first of all a working hypothesis, and the only one which can be followed up in all its consequences in the present state of science. And as I have said of our analyses and of our distinctions, so I now say of our working hypotheses: we have no right to regard it as a mere accident, that the world can only be exactly known if we apply just these working hypotheses. A system of metaphysics which would construct a view of the world without any regard to the working hypotheses which have been necessary, would be of no philosophical value.

IV.

As psychology is synthetical as compared with physical science, so it is analytical as compared with historical and ethical science. Historical science treats of human works, ethical science of human ideals, but psychology treats of the elements and of the general laws of mental life. The relation of psychology to historical and ethical science is dependent on the relation between elements and works and ideals. There are here three lines of thought which may develop side by side. They all draw from the same deep source: from the immediate and spontaneous mental life, the real and concrete life, which no analysis can exhaust, and which can never be expressed completely in any work or any ideal, as little as in any sum of elements. All research has here as its subject the infinitely con-

crete totality, and tries from different points to describe its nature and to express its fullness in definite forms. But the tones of life are so manifold and lie so close together, that no scientific notation can express them completely. This is as true with regard to historical and ethical science as with regard to mental science. But within this identical position there is an interaction between mental, historical and ethical science. If we want to find out the elements and laws of mental life it is not enough to study the single individual in its special states. A study is also required of human works and ideals, in which the nature of mental life is revealed throughout the ages. There exists no mental life in general. It appears in different forms at different times and places, and it strives to develop itself as fully as possible in every one of these forms, though the totality of its elements has a different timbre in every special case. Here psychology has a large amount of material for its analysis. The sociological method in psychology works side by side with the introspective, the experimental and the physiological methods. Mental science has a more abstract character than historical and ethical science, because elements are more abstract than works and ideals. Psychology here ought to apply the inversely deductive method, as it has been already applied by Comte and described by Stuart Mill.

The first step is to point out the process which has led to the rise of a work or of an ideal; the second is to deduce and explain this process from general laws of the interaction of mental elements. By pure deduction no results can here be arrived at. Reduction, not deduction, is what we can use. This is not only the relation of psychology to the historical and ethical sciences, but also to art—to the art of education, to the fine arts, and to the great art of ethical life. We cannot deduce pedagogics, æsthetics and practical ethics from psychology. But we can observe the spontaneous development of the art of education, of æsthetic production and of ethical life, and the ideals and points of view which are revealed in this development may be understood by the help of general psychological laws. And this is after all also the relation of psychology to the theory of knowledge and to the philosophy of religion. It has to show the

psychological possibility of the forms of thought which are presupposed in scientific knowledge. And it has to analyze the mental experiences of religious life. As to this last point I have expressed myself in the following manner in my *Philosophy of Religion:* "In Religion men have made some of their deepest and most intensive mental experiences. If religion is genuine and original, all the elements of mental life are at work in it with an energy and interplay not to be found in any other domain. The study of religious life is therefore of great importance to general psychology." Lastly, a reciprocal relation will more strongly establish itself here, so that the understanding of mental elements and of the laws of their activity will be able to guide and clear up the work in the special domains. Indeed, the history of these domains shows that directly or indirectly such an influence has always manifested itself. If psychology is to have a future, this influence will be still more important than it has hitherto been. Psychology stands in a great debt to its neighboring sciences, and to the different kinds of art. Let us hope, that it may be able to pay a part of the debt, though this debt ought always to be contracted again, if psychology, as well as the other sciences, is to make progress!

COMPARATIVE AND GENETIC PSYCHOLOGY.[1]

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The central purpose of this congress is the unification of knowledge, and the discussion of those general principles and fundamental conceptions which underlie the related problems of allied sciences. Now comparative and genetic psychology takes its place between biology, on the one hand, with its doctrine of variation and elimination, with its organic values in terms of survival, and, on the other hand, such normative sciences as ethics and æsthetics, with their doctrines of worth for the ideal life of man. In any case the starting point is in close touch with purely biological reactions, and the goal is our systems of knowledge and our ethical conceptions. And the fundamental principle underlying and giving unity to these departments of study, in their genetic and strictly scientific aspect, is evolution.

It may be well at the outset to state that the province of comparative and genetic psychology, as I conceive it, is to investigate the nature and mode of development of mental processes, dealing with them in their synthetic rather than their analytic aspect, at any rate employing the methods of comparison and analysis with a predominantly synthetic aim and in such wise as to enable us to reach general principles which may be applied to the elucidation of particular cases. Incidentally it may have occasion to classify mental products, to distinguish and group certain modes of instinctive behavior, to mark off from each sundry other types of association, and so forth; but it only does so in strict subservience to its central aim and object. That aim is explanatory rather than descriptive. Every piece of comparative and genetic work should be so planned as to contribute something to the establishment or the support of the principles of psychology. It should add fresh ideas to the ideal con-

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struction of the science. Only on these terms can we claim, and shall we receive, the cordial recognition of those who are working in other fields of psychological research — on these terms and as a matter of course, on those of constant and faithful appeal to the facts of observation and a rigid adherence to the canons of scientific interpretation. If we undertake our work in this spirit and with no narrower aim, the whole psychological brotherhood will gladly admit that such research is indispensable and of lasting value. In any case it should be our aim, in this section, to contribute to the basal principles of psychology by employing comprehensively the comparative method, and by special enquiries in the field of development and evolution.

My own studies, as some of my hearers may know, have lain in close relation to certain aspects of biological investigation, and I may perhaps assume that I shall be expected to respond to the honor done me by the invitation to speak on this occasion, by indicating in broad outlines some of the conclusions to which I have been led in so far as they bear upon psychological genesis. If then I be asked to give expression to one or two of the most salient points which strike one who approaches psychology from the biological side as of cardinal importance, I should perhaps place first and foremost as genetically fundamental the way in which, in the lower ranges of mental development and evolution, everything hinges on practical behavior and activity. Psychological process is indeed a middle term between the results of complex stimuli from the environment on the one hand, and the results of complex reactions to that environment on the other hand. But in the earlier stages of genetic process this middle term is wholly subservient to the practical needs of an eminently active and practical life. It does not attain a position of relative independence. It is never divorced from its natural outcome in behavior. It does not assume that peculiar and, from the purely biological standpoint, abnormal preëminence which it is apt to assume in the treatment of a predominantly intellectualist psychology of the earlier school founded mainly on the mental processes and products of philosophers and sages.

A second point which comparative and genetic study brings

out with almost equal clearness is the complexity of the biological foundations on which the beginnings of the psychology of the individual are laid, and the consequent fact that, in individual genesis, the initial data are already-grouped wholes and not sporadic and isolated sensation-elements. One of the problems which the earlier psychology essayed to solve is by what process of coalescence and elaboration isolated sensations could build themselves up into the complex wholes of perception and how these could relate themselves with the similarly-built complex wholes presented to consciousness when active movements were carried out. It assumed that the several sensations which may be distinguished through the application of a difficult and prolonged process of analysis and abstraction, were independent psychological units separately given, and sought to render an account of the manner in which these mental elements threaded themselves on the strands of association. A biological treatment has more and more clearly tended to emphasize the fact that the individual organism comes into the world as a going concern the recipient of groups of stimuli giving psychological net-results, on the one hand, and capable, on the other hand, on purely organic grounds, of complex modes of behavior which supply also their net-results, the two sets of net-results, coalescing so as to constitute felt unity-wholes. It has thus tended to relegate many of the problems of mental ontogenesis to biology, and has come to regard association itself as in large degree dependent on factors which are primarily organic and physiological.

If this be so, the starting point of genetic study lies in the borderland region where distinctively biological evolution passes up into, and is increasingly influenced by, psychological development. And for this reason many observers have selected the phenomena of instinct as most likely to throw light upon some of the lower phases of psychogenesis.

From the phylogenetic point of view we are at present, I fear, very much in the dark as to the earliest stages in the evolution of effective consciousness as capable of exercising, in association with its physiological concomitants, guidance in the course of behavior. By effective consciousness I mean that

which does, in some way, control organic activities. The only criterion we have of its presence is the observable fact that the organism profits by individual experience. It is, I admit, a difficult criterion to apply. But I confess that I am disposed to regard the introduction of consciousness into an ideal scheme of explanation without the application of some such criterion — the introduction it would sometimes seem of a sort of consciousness which is not a mode of experience — as a bit of mythology, which is harmful rather than helpful. Of effective consciousness, however, having demonstrably a guiding value, there is no evidence in plants. Dr. Jennings finds little or no sign of it in the lower Infusoria; nor can Dr. Yerkes find much or any proof of its presence in the Medusa. This is the region of tropisms and chemism and the like. Just when and how effective consciousness first comes into play we are, as yet, scarcely in a position to determine. Hence the problem has to be attacked in the main ontogenetically, by considering the connection between automatic behavior and that which affords evidence of conscious control, in organisms which exhibit both, each in relation to the other.

If there is one feature which is essentially characteristic of the popular conception of the influence of mind in the conduct of affairs, it is that effective consciousness is a controlling influence standing in some way apart from the organic happenings over which its control is exercised. Is this popular conception wholly without scientific foundation and erroneous, as some physiologists would assure us? Take any simple case of accommodation to circumstances through a modification of behavior due to pleasureable or painful experience in like cases — let us say the avoidance of nauseous insects by young birds — or any other example of the intelligent control of instinctive procedure. Can we conceive how the feeling-tone, as the concomitant of nervous processes involved in the instinctive procedure as such, could modify the direction of the discharge — could either augment or inhibit it. I for one am completely incapable of doing so. Reduce what we may suppose to take place to the simplest schematic form. A stimulus excites a nerve-center and the excited nerve-center distributes a response. I am utterly

unable to see how any conscious concomitant of the physiological action of that nerve-center, *per se*, can in any way influence the response. Something must be added which in some way influences the discharge; and this is what we term experience, embodied in other nerve-centers, or in parts differentiated from the automatic centers.

It seems to me, therefore, that we are inevitably forced to assume that the physiological foundation of conscious guidance is, in organisms possessed of a nervous system, a differentiation of control-centers from the centers concerned in automatic response; and that the ascent of mind is the concomitant of the evolution of a differentiated control-system which, during individual life, is constantly playing down upon the system which is concerned in merely organic reflex acts biologically coördinated as instinctive procedure. It is between these two systems, thus differentiated, that interaction takes place.

According to this conception the control-system plays the part of environment to the automatic system which is the physiological mechanism for purely organic adjustment; and this harmonizes with the popular conception of mind as a selective environment. And the characteristic of this environment is that it includes, as modes of experience, on the one hand the surrounding life-circumstances, and on the other hand the responsive organic activities, and brings them into those relationships which we term psychological values. It is this controlling environment which is constantly influencing the course of procedure due to the hereditary modes of response of the automatic centers.

I am well aware that this conception of an environment within the organism itself runs counter to established usage of the term. It will be said that the word implies those external conditions to which the organism as a whole is adapted through heredity or accommodated through acquired modifications of structure or function. It will be urged that it is this external environment with which the control-system is in relation, and that the suggestion I put forward involves an unwarrantable departure from all the recognized canons of biological interpretation. And yet, having all this in view, I venture to put for-

ward the conception in the interests of psychological interpretation. There is not time now adequately to discuss it, even were this the appropriate occasion; only the salient features can be indicated and that very briefly. The determining conditions of psychologically-guided or intelligent behavior, as distinguished from responses which are purely automatic, are what we sum up under the term experience. It is commonly said that this experience is that which stands for, or represents, or symbolizes the environment. I wish to suggest that it *is* the psychological environment under the influence of which automatic responses and instinctive modes of precedure are modified, and that in all cases it includes more than the actual presentations of the environment as that term is used by the biologist. It includes the meaning which that environment has acquired. A chick that has had some acquaintance with the nature of wasps inhibits the instinctive tendency to pick at one when it is presented to sight. That and that alone is the presentation of the external environment at the moment when inhibition is brought into play. That and that alone is not the determining factor in the intelligent avoidance of the insect. This controlling factor is the meaning within experience which the presentation suggests. It may be said that what is suggested is a potentiality of the external environment. But the controlling influence of potentialities is hardly a satisfactory conception. What is actually present then and there is the experience, modifying the output of automatic response. This is to be regarded, according to the suggestion I put forward, as the psychological environment. But it is physiologically embodied in the control system which is the actually present material environment under which the further functioning of the automatic centers is conditioned in intelligent behavior. The essential points then are these: (1) Experience, insofar as it controls behavior, may be regarded as the environment which supplies the conditions of guidance; (2) what the biologist terms the environment is a product of experience; (3) for the physiologist experience must be translated into its neural concomitants in the control system; (4) hence, if, psychologically, experience may be regarded as a conditioning environment, then, physiologically, the control system, as its organic embodiment, may be so regarded.

Now there are diversities of opinion as to the range which should be included under a definition of instinct as contrasted with intelligence, and there are diversities of opinion as to the relations of the one to the other in genetic process, especially as to how far the modifications of behavior produced through the exercise of environing intelligence are directly inherited as variations of instinctive endowment. I do not propose to discuss the *pros* and *cons* of that difficult subject the inheritance of acquired characters. It suffices to say that in accordance with an hypothesis in the development of which I am proud to be associated with Prof. Mark Baldwin and Prof. Henry F. Osborn, intelligent modification of behavior, if it be not the mother of congenital variations of hereditary instinct, may none the less be regarded as their fostering nurse.

This is not the occasion, however, on which to discuss diversities of opinion or indeed to enter, in any detail, into the more distinctively biological aspect of the study of instinct. From our point of view the essential feature of instinctive procedure lies in the fact that the behavior thus characterized is on its initial occurrence prior to and independent of individual experience. It wholly depends, as such, upon how the automatic centers have been built through heredity. And from the standpoint of genetic psychology it appears to me that the really important contribution which the study of instinct offers for our consideration is this: that in any given case of hereditary behavior what we may term an instinctive situation is presented to consciousness, as, ontogenetically, a primary unit-complex of experience, and that, as such, it is developed independently of any guidance in terms of experience. By the situation as presented to the environing consciousness I understand the whole of the initial stimulation, including both external and internal factors, the net results of the behavior as the situation develops, and the satisfaction or dissatisfaction which is attached thereto. We, as psychologists, analyze the instinctive situation. But I conceive that it is presented to consciousness as one developing whole. And the mode of its development is an organic legacy; it is essentially a flow of physiological process in the automatic centers; but it entails a flow of consciousness in the environing

control centers; and this flow of consciousness in its entirety, within a given situation, I am disposed to regard as a primary datum in ontogenetic development.

This thesis, which I purposely express in a somewhat extreme form for the sake of emphasis, involves a protest, I do not say against a too analytic treatment of the early phases of mental process (for it is our function to analyze and compare) — but against the assumption that the products of our analysis are, psychologically considered, genetic units. We sometimes fail to realize to how great an extent we are apt to become the slaves of the disintegrating tendencies of scientific procedure. Because we can break up a situation into what we call its constituent elements, we think that they are separately felt elements in the primary experience. I do not think that this is the case. I regard it as much more probable that the developing situation is collectively felt as it is unfolded; and that complex wholes, biologically integrated, rather than constituent elements, analytically disintegrated, are for ontogenetic treatment the primary data.

On this view, then, instinctive procedure presents to the environing consciousness, embodied in the control system, ready-made situations. And, on the subsequent occurrence of like situations, under substantially similar circumstances, these are dealt with in accordance with the meaning which their predecessors had acquired.

One can not however too strongly emphasize the fact that, in passing from biological responses and reactions, to conscious behavior founded on experience, we introduce a wholly new order of values — values not in terms of organic survival but in terms of feeling-tone. The two sets of values are so often and, of necessity, so predominantly consonant — their inter-relations, are so many and so close, that we are apt to forget that they are radically distinct. Physiology, as such, knows nothing whatever of that order of pleasure-pain values, which for us, as psychologists, are essential. They form no part of the ideal construction of physiology: they are dominant factors in the ideal construction of psychology.

And it is here, just where the strictly biological and the dis-

tinctly psychological factors begin to interact, that the difficulties of analysis make themselves felt. I have distinguished between the automatic system, the functioning of which is determined entirely by biological values in terms of survival; and the control system, the functioning of which in its psychological aspect is determined entirely by a different order of values in terms of feeling-tone. The outcome of the one is instinctive behavior; the outcome of the other is intelligent behavior. But both are dependent on heredity. And it is therefore, I think, essential to distinguish, in our ideal construction, between two orders of heredity: first, that which obtains within the automatic system and which thus determines the nature of the hereditary responses; secondly, that which obtains within the control system and which thus determines the nature of the hereditary likes and dislikes. For analysis these are independent each within its appropriate sphere; but they are developed within the same organism in close synthetic relationship.

At the outset of ontogenetic development instinctive and automatic responses are due to the purely biological order of heredity; but their results are reflected in the conscious environment and therein are subject to the psychological order of heredity so that the controlling influence of the environment is determined by feeling-tone and values for conscious experience. If then we speak of the development of a situation in conformity with the satisfaction it affords, as in accordance with the psychological end, and its development in conformity with the preservation and conservation of the race as in accordance with the biological end, the salient fact is that the two ends are consonant. This has, of course, been fully recognized by evolutionists from Herbert Spencer onwards. I will not here lay stress upon the noteworthy fact, which has not, I think, been sufficiently recognized by the Lamarckian school of evolutionists, that this consonance of biological and psychological end, is admitted to be the outcome of the survival of those in which the consonance obtained, and the elimination of those in which it was absent—that is to say is admitted to be dependent on natural selection. I would rather lay stress upon the fact that this consonance affords a striking link of continuity between the more distinc-

tively biological and the more distinctively psychological factors of the genetic process.

The relation between the two has been well brought out in Professor Groos' discussion of the so-called play of animals. Indeed such play admirably illustrates the two-fold influence of heredity; for on the one hand it is founded on unquestionably instinctive modes of behavior; and on the other hand it not less obviously appeals to an innate sense of satisfaction. Why do animals begin to play and keep on playing? From the psychological point of view because they like it: from the biological point of view because they thus gain practice and preparation for the serious business of their after life. But why do they like it? Because under natural selection, those who did not like it, and therefore did not undergo the preparatory training and discipline of play, proved unfit for life's sterner struggle, and have been therefore eliminated. I have contended that inherited modes of behavior present to consciousness ready-made situations which develop automatically on biological lines, and that the rôle of environing intelligence is to lead to modifications in their redevelopment in accordance with their psychological values. I have also called to remembrance the fact that in the animal world, under normal conditions, these psychological values with their appeal to feeling are consonant with biological values in terms of survival. Throughout the course of mental development, in the perceptual sphere, there is a constant interaction between the two factors broadly classed under the heads, 'instinct' and 'intelligence.' And it is the province of detailed study to assess at their true value the rôles played by these two factors in the particular cases which fall under consideration. A biological survey of the field discloses the fact that when the conditions of life are constant and uniform the instinctive factor, subject to organic selection, predominates and complex groups and trains of reflexes assume a stereotyped form. This is due to what we may term a biological coalescence of unit situations into a coördinated whole. What we call the instinctive behavior of many of the insects for example seems to be the inherited grouping in biological sequence of inherited units of response. But in relation to more varied circumstances the

intelligent factor predominates. The hereditary unit-situations may not be fewer, may indeed be more numerous. But the manner of their coalescence and coördination is rather psychological than biological. The higher animals exhibit an intelligent plasticity which enables them to meet the requirements of the more complex surroundings into which their life has risen, and which is reflected or symbolized by the psychological environment. Here a stereotyped coördination of the hereditary units of behavior would be rather a hindrance than an advantage. The winning animal in life's struggle would be the one in which behavior was most rapidly and most surely modified to meet particular needs — the one in which the teachings of experience were most promptly used in effective action. The inevitable tendency of the evolution of intelligence must be the disintegration of stereotyped modes of behavior as biologically coördinated wholes and the dissolution of instinctive complexes into relatively independent instinct units which would be thus free to coalesce into new groups under the guidance of experience. Thus it is that in the more highly developed animals, and in the human subject, the instinctive units assume rather the form of a number of congenital tendencies or propensities than of instinctively coördinated wholes of behavior, the former being less stereotyped than the latter. And thus it is that in them there is a shorter or longer period of inefficiency during which the inherited unit-situations are coördinated psychologically in new groups under the influence of individual experience as a shaping environment.

Throughout the whole range of perceptual development under these conditions there is progressive integration and differentiation of the unit situations, always on essentially practical lines, always in closest touch with active behavior. Even perception itself, as genetic psychology has helped us more fully to realize, is dependent on acquired habits of action. Perceptual meaning and value are ever dependent on some activity directed toward that which is so perceived. All differentiations within the presented situations are due to the call for some directed behavior, are due to the demand for some focussing of active manipulation. Thus is the mouse differentiated for the

practical interests of the kitten. And all integration of diverse situations is due to their assimilation in terms of like modes of behavior in dealing with them, in terms of the similar responses which they evoke. Thus there is an integration of the situations of so-called play and earnest. But in perceptual process, far as differentiation may be carried, it never reaches the stage of intentional analysis; and, far as integration through assimilation may be carried, it never reaches the level of intentional generalization. These are the results of ideational process.

It will be noticed that I here lay marked stress on the distinction between perceptual and ideational process. I said at the outset that comparative and genetic psychology takes its place between biology on the one hand, with its doctrine of values for organic survival, and, on the other hand, such normative sciences as ethics and æsthetics with their doctrines of worth for the ideal life of man. It appears to me that in the relation of biology to psychology the essential point is to grasp the analytic distinction between the instinctive and intelligent factors, and I have therefore so far mainly dealt with this distinction. But it also appears to me that in the relations of psychology to normative science, the equally essential point is to grasp the distinction between perceptual and ideational process. In the study of the higher ranges of animal psychology and of child life this distinction has scarcely yet received adequate emphasis.

The study of the mental processes of the higher animals has of late years passed into a new phase. In the first place it is now realized that, so far from being easy it is full of peculiar difficulties and beset with special snares which entrap the unwary interpreter. In the second place it is generally admitted that adequate training is required to enable an observer, no matter how accurate and faithful his record of facts may be, to diagnose inferentially the psychological conditions of which the facts themselves are significant. And in the third place it is recognized that far more is to be gained by the systematic study of the doings of animals under controlled conditions and in test cases, than by the casual observations of credible but often uncritical witnesses. The new phase of the study of animal intel-

ligence is thus characterized by experimental research in the hands of those who are trained psychologists, and who are fully aware of the difficulty and delicacy of the task which they undertake.

We must remember that in the early days of Darwinism, the first business of those who sought to place the conception of mental evolution on a secure basis was to establish the basal principle of continuity in the series of mental products; to show that in animals are to be found the germs of all the higher endowments of man; and to abolish all such radical distinctions in kind as were then held to form an impassable gulf between the brute beasts and man with his spiritual capacities. This tendency, which was inevitable as a stage in the progress of thought, led to the utmost widening of the limits within which psychological terms, such as 'inference,' 'abstraction,' 'generalization' and 'reasoning' were used; the extension being designed so as to render these terms as comprehensive as possible and to enable them to cover not only fully differentiated processes but the earlier — even the embryonic — stages of their development. But when the conception of evolution had won its way to acceptance, when the principle of continuity had taken its place as part of the recognized scheme of scientific interpretation, the emphasis of thought changed from the evolutional curve as a whole, now freely and fully accepted as continuous, to the differentiated stages which could analytically be distinguished therein. They were applied again in more restricted senses; the restriction being designed to render them distinctively applicable to certain higher phases of differentiation within an admittedly continuous process.

In any case it is necessary to bear in mind the fact (of which I have suggested the probable cause) that the same terms are applied by different authors with wide differences of limitation — by some in a more extended and by others in a more restricted sense. From this it follows that some at least of the divergences of interpretation in the comparative psychology of animals are more apparent than real.

The influence of the terms we employ, closely connected as it is with our early training, is often deep and abiding. It has

been a special merit of Dr. Stout's treatment of psychological topics that he has emphasized, so clearly and in so many ways, the fundamental distinction, as I conceive it to be, between perceptual and ideational process. As he himself has pointed out, one of the great difficulties in the way of its general acceptance, is due to the fact that the existing terminology grew up at a time previous to any serious attempt to render clear the distinction. Some of my hearers may remember the almost pathetic words in which Dr. Stout laments the misleading influence of the terms we are at present almost forced to employ. If I may be allowed slightly to modify his statement without, as I believe, introducing anything foreign to his thought, his contention is that "human language is especially constructed to describe the mental processes of human beings [in ideational terms] and this means that it is especially constructed so as to mislead us when we attempt to describe the workings of minds which differ in any great degree from the human" and even the workings of our own minds on the perceptual plane. "A horse having had a feed at a certain place one day, stops of his own accord at that place on a second journey. People say that it remembers being fed here before, and infers that it will be fed here again. In all probability these words with their human implications [on the ideational plane] are quite misleading. Suppose that the master of the horse is a bibulous person, who takes a drink as a matter of course whenever he comes to a public house on the road. In order to do this he need not go through the process of remembering that he has had a drink at a public-house before, or of inferring that he can have a drink at a public-house again. He simply has a bias to stop at a public-house whenever he comes to one. Probably the horse's act implies just as little of remembering or inferring."

It will be noticed that the difficulty which Dr. Stout indicates, does not apply only to the mental processes of the horse, but also to some at least of those which are characteristic of his bibulous master. No doubt, taking men and women as we find them, there is the closest interaction between ideational and perceptual process, just as there is between instinctive and intelligent procedure. But there is, I conceive, an analogous

relation. Just as the instinctive factor provides data which intelligence deals with so as to shape it to more adaptive ends, so does the perceptual factor provide the more complex data which, through ideational process, are raised to a yet higher level in rational conduct. And in both cases notwithstanding, nay largely in consequence of, the closeness of the interaction it is the business of analysis to distinguish with the utmost clearness the essential features of the constituent factors.

I take it that the leading characteristic of perceptual process is the dealing with situations as wholes in their unanalyzed entirety. When the integration of which I have spoken has been carried far, any relatively new situation is assimilated to the past experience gained in similar situations wherein certain salient features have been differentiated through their intimate relation to practical activities. The associations thus begotten are not associations between separate ideas but in every case essentially between the situation and the practical behavior it calls forth. Even this expression savors too much of analysis. Let us rather say that the type of association distinctive of perceptual process is that between an early phase of a situation and the succeeding phase, so that what is suggested in any given case is a mode of development of the situation as a whole through practical behavior. That is the essential feature of the doctrine of meaning in perceptual process. It is meaning in terms of a specific development of the situation as a whole; it is meaning closely bound up with a felt impulse to act in a certain way; it is the meaning which attaches to the public-house as the result of practical experience on the part of the horse and of his bibulous master.

Now, it appears to me that recent researches all point to the fact that the mental processes of animals are mainly — I do not say entirely, though I myself still incline to that opinion — but at all events mainly, on the perceptual plane. They tend to show that animals, even the monkeys, deal with situations as complex unity-wholes. The method of learning is chiefly dependent on practical behavior which, carried out with varied and persistent — often restless — activity, leads the animal unsystematically to stumble on new associations between such behavior

and the situation within which it arises. But it also appears to me that a very large proportion of human process is predominantly upon the perceptual plane. I say 'predominantly' because even this section of human activity is inevitably influenced by the ideational section which is superinduced thereon. And there is, I repeat, no little difficulty in determining its range, as perceptual, just because our psychological language almost necessarily leads us to describe it in ideational terms — the terms begotten of comparison, analysis, and synthesis.

It is through such steps, and such steps alone, that on the basis of perceptual experience, systems of knowledge can be built. This is the product of ideational process. It involves an ideal or schematic construction. And when situations are viewed from the standpoint of a system of knowledge their salient features have not only meaning for practical behavior, but also significance in relation to that system. They are apperceived as particular examples which illustrate some general scheme or principle. They are subject to the influence of a new environment. And it is here that psychology comes into touch with normative science. No doubt, normative science, as its name implies, deals with standards of reference — in ethics, for example, with standards of 'ought.' But this is only an implication of the fact that the particular act is viewed in its relation to an ethical scheme of conduct. Impulses arise within the situations as they occur and as they are dealt with in and for themselves. But motives, as the term is used in ethics, imply the relations between these several situations and a system of ideals. Only on the ideational plane do there emerge considerations looming up beyond the situations into a prudential, moral or other scheme; behavior is thus raised to the level of conduct; and a situation is developed, not only in accordance with the impulse-value arising therein, but in accordance also, and in greater degree, with the motive-worth for a system.

One of the characteristics of ideational, as contrasted with perceptual reference is that, whereas in the latter the salient feature of the situation is always the center of development, in the former the comparison is with independent centers, or an independent scheme, to which many situations may be referred. This

may be illustrated by the different attitude of the two types of process to relationships in the surrounding world. For perceptual process these relationships are not disentangled in analysis from the complex within which they are embedded, nor are they synthetically rebuilt into new unity-wholes; but they are none the less contributory, within the complex, to practical behavior. Suppose one is shown into a strange room and deals with the situation in purely perceptual fashion. One assimulates the various impressions in terms of past experience. At any given moment some object — say the clock on the mantel shelf — is in the focus of vision. The others are grouped around it in the margin of the visual field — the picture above, the fire-place below, the bookcase on one side, the cabinet on the other. These are the spatial relationships within the field for perceptual experience. But in and for the situation as such the center of reference is always the focus of the field. It is a constantly shifting center. As the eyes flit from object to object the focal impression changes again and again. And with each change the space-relations of other objects, more dimly seen in the margin of vision, are rearranged. But always for the situation itself, as contrasted with systematic knowledge, the center of reference is the focus of vision (or other mode of perceptual experience) at the time being. It is an essentially practical center. It *means* that so much movement of the eyes or hands or body as a whole in this or that direction will bring this or that impression, at present marginal, into the focus. Practically perceptual folk constantly tend to deal with a spatial situation in this way. They picture how they would act in the midst of it. Ask a farmer's lad whereabouts the church is in his native village. He will probably reply that as you leave the Blue Boar public house (his focal point) you must first turn this way, then that way, and then go straight down the street till you get there. The distinguishing feature is that the spatial relationships which are disentangled in ideational process for purposes of schematic construction and rebuilt in a conceptual scheme of three dimensions — a scheme independent of, but applicable to, particular situations — are for experience on the perceptual plane parts of a given complex, having meaning for practical behavior, but as

yet no significance for systematic thought. They are not subject to the influence of an environment of ideational constructions.

For we are now in a position to extend the conception of conscious environment. Intelligence, in the perceptual sphere, embodying the coalescent representation of concrete situations plays the part, as I suggested, of environment to the automatic responses. And this undergoes evolution to higher and higher levels in perceptual process. But in ideational process there is superimposed a further environment under the influence of which intelligent procedure is itself controlled. This higher environment is constituted by systems of knowledge, ideals of conduct, and artistic conceptions. Just as intelligence fulfilling its function of environment, plays down upon organic procedure, shaping it to more perfect adjustment to the circumstances of perceptual life, so does reason, as environment, play down upon intelligent procedure, moulding it to more perfect adjustment to the conditions of ideational life.

According to this conception, there are superimposed upon the pleasure-pain values in terms of feeling-tone, yet higher motive values of a new order in terms of logical, ethical, or æsthetic worth. And if we attempt to translate this into physiological terms, not only is there a differentiation of a control system from the automatic nerve centers, there should also be a further differentiation within the control system itself, yet higher intellectual centers being differentiated from those which are concerned in perceptual process.

There is however a further distinction which is important for the comprehension of the social influence of ideational process. Perceptual intelligence is, in the main, receptive and representative of a natural environment which takes form independently of the exercise of its influence. Only in a limited degree are its products in behavior so applied as to modify and enrich that natural environment. The beaver indeed constructs its dams, the bird builds its nest, the spider spins its web, and so forth. Some amount of choice of environment through subjective selection is possible. Some products of behavior are projected on to the plane of its organic or inorganic surroundings. But it is a characteristic feature of ideational process that it is constantly,

in an indefinitely larger degree, embodying the products of its rational environment, as developed in consciousness, in concrete form so as to constitute part of the physical and intellectual surroundings. Subjective selection is a most potent factor in human life. To an extent only foreshadowed in the animal world does man both create and select his own environment of circumstance. And this is the keynote of the higher human evolution as contrasted with that which obtains among the lower animals. It involves a transference of evolution from the organism to the social environment.

Now if the distinction between perceptual and ideational process is sound in principle, it is of fundamental importance in dealing with the higher ranges of psychological development. In any case, whether the subject be man or child, dog or monkey, it is our duty to devise such methods of observation as shall enable us to apply these principles in such a way as to analyze out the two factors, supposing that both coexist. As I read the results of the recent researches of Dr. Kinnaman, Professor Thorndike, Mr. Hobhouse and others, on the intelligence of the primates, there seems however, but little evidence, even in them, of the schematic products of ideational construction. And the question arises whether such products are possible without language as an instrument of analysis and synthesis.

In attempting to deal, I fear very inadequately, with the subject committed to my charge, I have essayed to give a very broad and general survey of the genetic sequence. Starting with biological reactions and bringing these into touch with the early stages of intelligent guidance, the essential feature is the occurrence of a new order of values, those of feeling-tone in terms of pleasure-pain. So long as situations are dealt with naïvely in and for themselves these values are the determining psychological factors, but always in close and vital relationships with the survival values of the biological mode of explanation. At some stage, however, of the evolutionary process a new order of values has its rise and origin — these are connected with ideal schemes and systems; and they are in terms of worth for the ideal life. Just as intelligence forms the environment under which automatic responses are guided to higher ends, so does

some sort of rational system form the environment under which perceptual processes are controlled. Here we have the scientific foundations of ethics. But here too, important as these scientific foundations may be, many of us feel that they are insufficient; and we are thus brought into close relations with those metaphysical postulates which are outside the sphere of comparative and genetic psychology, quâ science — with relations which no doubt have been or will be discussed in another section of this comprehensive Congress of Arts and Science.

MENTAL PATHOLOGY.[1]

BY PROFESSOR PIERRE JANET,

Collège de France.

GENTLEMEN: I feel that it is a great honor to be called upon to address the Section of Pathological Psychology in the St. Louis Congress. The United States has done much for psychology. Your magnificent laboratories, your important publications, and your eminent men who have devoted themselves in great numbers to psychological investigation, have contributed abundantly to the development of the science. We are pleased to come and admire your work; we are proud to bring to you the results of our own investigations.

I am the more encouraged to present to you the greetings of the psychologists of France, by the fact that we have been concerned chiefly with the somewhat specialized topic of pathological or abnormal psychology to which this section of the Congress is to be devoted. If I mistake not, the investigators of other countries tend to separate two branches of study which we are disposed to unite; they study on the one hand, the psychology of the normal individual, or the individual who is regarded as being normal, and on the other hand, they are concerned with mental diseases, their analysis and especially their classification. It seems to me that we in France, under the influence of two of my masters, Ribot and Charcot, whose names I am pleased to recall to you, have endeavored rather to throw light upon psychiatry by a study of normal psychology; and to regard mental diseases as experiments which have been cunningly devised by nature to show us such suppressions and modifications of function as the experimental method demands. Our psychological laboratories, situated as they often are in hospitals for nervous and mental diseases, as at the Salpétrèire, at Ste. Anne and at

[1] Address delivered at the Congress of Arts and Science, St. Louis. Translated by J. W. Baird, Johns Hopkins University.

Villejuif, have endeavored to unite the psychologist and the alienist in a common investigation.

This union, alike in France and in other countries where it has also been accomplished, seems to me to have been advantageous to both sciences. In psychiatry it has to some extent turned the investigator aside from investigations which are actually useless because they cannot be utilized. One is scarcely able, even to-day, to make a complete classification of mental disorders from a single point of view as is required by logic. As Ziehen has remarked, mental diseases are classified differently from the standpoint of symptoms, of etiology, of evolution and of pathological anatomy. Moreover, it must be confessed that we do not know the real causes of mental diseases; and it is bootless to disguise our ignorance by a cloak of philosophical speculation or of hypothetical anatomy. The pathological psychologist has recognized that he must begin at the beginning. He has endeavored to penetrate more thoroughly and more sympathetically into the mental states of the diseased; he has attained greater accuracy in the analysis of symptoms; he has observed and, so far as possible, has measured the alterations of psychical function. In short, psychological experimentation has introduced into psychiatry a rehabilitation and a refinement of the clinical method.

The older psychology, permeated as it was throughout with philosophical speculation, claimed to find that the mental states were as simple and as unchanging as its theories. It studied memory in general, reason in general, the theoretical and abstract will, without first taking up the question as to what constitutes memory, reason and will in a particular individual, under particular conditions, at a particular age, in a particular state of health. The investigation of pathological conditions has forced it to recognize that these phenomena are not fixed and immutable; it has come to see that they wax and wane, that they are subject to change, and that a multitude of degrees are represented in their developmental transformations. Psychology has thus been led to seek, even in the normal individual, for those changes and oscillations which it has found to be characteristic of the abnormal individual; it is no longer abstract, but has

become a more real and living thing. If one may use the expression, it has ceased to be purely static, and has become dynamic. The study of the oscillations of mind which have been brought to light by pathological psychology, has called attention to a group of phenomena which can scarcely be classified among the older faculties, and have been neglected as being of no significance — the phenomena of fatigue, of sleep, of the emotions, of the various forms of intoxication, of the neuropathic disorders. It has also called attention to the opposite modifications which occur in repose, in the waking state, in calmness and *sang froid*, and in convalescence (even though it be but temporary). These modifications constitute the most apparent oscillations of mind. Permit me to lay emphasis upon these depressions and these excitations, and to remind you of the investigations which have been made in this connection. The topics which have not yet been investigated are of even greater significance; they represent the most important of the present problems of pathological psychology.

One of the first modifications of mental states to which I wish to call attention, is illustrated by the semi-normal, semi-pathological condition which is induced by fatigue. It is a familiar fact that the physical and mental being does not always maintain a constant condition; that it is incapable of manifesting uniform phenomena when submitted to prolonged effort; that its functions vary from the beginning to the end of a period of work. And the change induced is essentially of the same character whether the work be physical or mental. The investigation of fatigue dates from an early period. You doubtless recall an old observation by Holland which has been cited by Ribot.[1] "An engineer relates the following experience: 'When I was down in a mine, I felt myself overcome with fatigue and lassitude to such a degree that I found it impossible to converse with the German inspector who accompanied me. Every word, every phrase of the German language had escaped my memory, and I recovered them only after taking nourishment and rest.'" Here we have a first well-marked opposition between the normal state and the state of fatigue. Oppositions of this sort have been the

[1]Ribot, *Maladies de la mémoire*, p. 114.

object of numerous investigations, among which may be mentioned Galton's interesting study of the influence of fatigue and overwork in schools, the investigations of a great many German authors, instigated for the most part by Kraepelin, the remarkable observations of the French physician Tissié, who made a physiological and psychological examination of the contestants in various forms of athletic sports, the investigations of Féré, of Binet and Henri, etc. All of these investigations show the presence of modifications of constant character.

It is to be noted first of all, that an apparent exaggeration of function occurs with the advent of fatigue. This functional change makes its appearance in the form of a physical agitation, or in other words, an exaggeration of movement. Galton found signs of fatigue when he examined the posture of a subject to whom a difficult passage was being read aloud. The fatigued auditor yawns, stretches himself, shifts his position and contracts certain facial muscles. In school-children one may observe movements of the eye-brows, of the lips, of the forehead, and of the fingers; and if the degree of fatigue be increased, these movements soon pass over into chorea and tic. Experimental investigations, such as those of Sommer and Bettmann, have revealed the presence of a modification of the reflexes and an increase in the number of tremors in the ergographic curve. Pathological investigations have shown an exaggeration of the reflexes, an extension of innervation to the unused muscles, an involuntary laugh, a muscular tremor, and spasms of various sorts, etc. The agitation induced by fatigue may also be visceral; I cannot lay too much stress upon the spasms of the digestive organs, the changes of respiratory rhythm, the profuse perspirations. The agitation may even be mental. Galton observed irritability, ill-humor, a tendency to magnify small things; and more recently, attention has been called to various forms of fancies which take possession of the mind and may even degenerate into pathological obsessions. The subject is well aware that something abnormal is taking place within him; he is conscious of certain abnormal sensations. Galton emphasized the feeling of incapacity which increases with increase of fatigue; Oehrn, in 1895, and more particularly Tissié laid stress upon

the feeling of weariness as a characteristic symptom of fatigue. These phenomena are observed alike in bicyclists, in children who do mental work and in subjects who repeatedly perform an experiment. These feelings correspond to something which is perfectly real; it is possible to demonstrate in various ways that a decrease of mental function runs parallel with the agitation. Whether we examine the subject's penmanship, or measure his capacity to insert a needle into holes in a perforated plate — as I did in 1889, and as Bryan has done since — we invariably find a lack of dexterity, a lesser precision of movement. The diminished rapidity which is found to be characteristic of reactions and of all sorts of motor adjustments has been demonstrated in innumerable ways (Kraepelin, Oehrn, Burgerstein, Vaschide, Binet and Henri). On the other hand, attention and power of apprehension also decrease in considerable degree; and one finds an increase in the number of errors in tasks whose accomplishment depends upon automatic mental processes, or the mechanical association of ideas (Cattell, Finzi, Sikorsky, Hopfner, Burgerstein, Laser, Thorndike, Binet). The memory undergoes a change, as we saw from the illustration cited by Ribot; acquisition becomes more difficult (Ebbinghaus, Finzi, Schneider); the power of recollection, the certainty and the correctness of response decrease (Ranschburg), and certain classes of memories disappear entirely.

The disturbance even extends to perception (Grote, Marine, Griesbach, Leuba). All of these phenomena are in striking contrast with those that occur in repose. This opposition between the mind in the state of rest and in the state of fatigue, is of prime importance for pathological psychology.

Similar phenomena are to be observed in the various forms of intoxication; here too is to be seen an interesting opposition between the mental state of the intoxicated and of the non-intoxicated individual. Numerous investigations, such as those upon haschish by Tours, those upon alcohol by Richet, and many others, have established phenomena which are analogous to the phenomena of fatigue. But I wish to direct your attention to a class of investigations which has reached a high degree of development in France — those of sleep, or rather of sleeps,

for there are many states to which this general name may be applied.

Sleep is a type of oscillation which is particularly deserving of notice in this connection, because it is a wholly relative condition; its phenomena can be determined only in relation to the waking state. An essential characteristic of sleep is the fact that it is attended by a lesser activity of the vital functions. It is not enough to say that sleep is a state in which the temperature of the body averages 36°, the pulmonary expiration is two liters, and the pulse is 54. One must add that these phenomena occur in an organism which is capable when in another condition, of having a temperature of 37°, a respiration of nine liters, and a pulse of 70. It may be said that the organism is unable to keep up its more active form of existence continuously, and that it practices economy during a part of its life. Nor is the oscillation solely physiological; it is mental as well. Dreams are the thoughts of the sleeping man. I need not remind you of all the investigations of dreams which have been made from the time of Hervey de Saint Denis and of Charma, down to the recent publication of Sante de Sanctis. Let me mention however that dreams are attended by a mental agitation which manifests itself in hallucinations, and in a ready association of images which arrange themselves into tableaux with interminable kaleidoscopic changes. Sense-impressions of slight intensity give rise to complex dreams of similar modality (Maury, Sergueyeff, Mourly-Vold). These dreams are characterized by exaggeration and repetition; the Cartesian flea-bite becomes a sword-thrust, and a trifling weight seems an Etna upon the chest. The same dream recurs countless times with wearying monotony. The memory of experiences long since past is vivified; the instincts and the habitual tendencies have free play and develop immoderately; even the hereditary tendencies may intervene in intense degree. It is true that the dreamer may feel that it is all unreal and fictitious, but he is carried along by the turbulence of his imagery, and he frequently experiences the most violent emotions from his images. Side by side with this exaggeration of certain mental functions there occurs a diminution of other mental functions; and some of the latter are most peculiar and extremely characteristic.

It is evident that the consciousness of personality is disordered, and that a duality of personality tends to rise. Charma and Delboeuf report dreams in which a school-master asks them a question; they are unable to answer but a school-mate rises at their side and to their astonishment gives the correct answer. In another case the dreamer says to a child: "Be careful that you do not tumble," and he himself slips. Thereupon the child replies: "Why don't you follow the advice which you give so freely to others." Again, a dreamer who has a pain in his head, meets a child who is also suffering from headache, and asks the child to suggest a remedy. Will and attention are wholly lacking in dreams. There is no real adaptation either to internal, to external or to future conditions. There is no resistance, no control, no criticism.

I should like to mention a particular form which the diminution of attention assumes in dreams. Several authors (Egger in France, Schneider in Germany) have pointed out that in dreams only the center of the mental picture is illuminated; the outlying parts are invisible, or rather they are non-existent. The pictures appear without any setting. And it is just this absence of surrounding objects, *i. e.*, of environment of thought, which explains the absence of comparison and criticism that is characteristic of dreams.

The study of one's memory of dreams reveals other interesting characteristics of the enfeeblement of attention. In the first place, dream experiences do not become firmly fixed upon the memory. When we awake we fail to remember what we have dreamed, and dreams which do not recur are forgotten as soon as they take place. This is the form of oblivescence which in another connection, I have called continuous amnesia, and it is interesting to note that it is to be found in dreams. But there is an even more peculiar feature; Delage, de Sanctis, and Pilcz have observed that the striking events and the intensive emotions of the day do not reappear in the dreams of the following night. The mother who has just lost a child is surprised to find that she does not dream of her loss although that subject has occupied her mind all day long. These authors furnish different explanations of this fact, which need not be

discussed here. Let us remark however that the oblivescence of recent experiences is well known under the name of retrogressive amnesia. In this disturbance, as in dreams, events reappear in memory only after they have long since been experienced. By way of summary, we may say that dreams are characterized by a narrowing of the field of consciousness, by continuous and by retrogressive amnesia.

Other phenomena which are equally semi-normal and semi-pathological, appear in the emotions. When an individual finds himself suddenly placed in a position to which he is not already adapted by previous habituation, when he lacks the time or the strength required to adapt himself to the new conditions, he experiences certain forms of physical and mental perturbation which are of prime importance. In this country where the James theory of the emotions was developed I need not discuss the emotional value of the visceral excitations. The increase of heart-beat and of respiration, and the spasms of the digestive organs, are well-known features of the state of emotion. It is also known that hunger assumes an exaggerated form in emotion, a phenomenon which in all probability gave rise to the custom of feasting at funerals. These internal excitations extend to the muscles of the members, and in many emotions one may observe an indefinite repetition of violent and useless movements, grimaces and convulsive contortions of all sorts. Numerous authors have been pleased to find in these incoördinated movements, a trace of more or less complete acts which are inhibited in their initial stages in the modern subject; that is, to regard them as vestigial products of movements which attained complete execution when our human or animal ancestors were exposed to similar conditions of stimulation. Stanley Hall's and Dewey's investigations of anger give us a great deal of information upon this point. Moreover it frequently happens that the tics, the various forms of tetanus and the impulses to flee or to cry out, remain undeveloped in the presence of emotional states.

But we must not confine ourselves to the peripheral manifestations of emotion. The weak point of the famous theory is to be found in the dictum that: 'We are sorry because we

cry'—an objection which has been urged by many authors (Irons, Gardiner, Soury, Dearborn, Sherrington, Baldwin and others). Side by side with these motor phenomena, which have quite correctly been called extra-motions, there occur intra-motions which constitute retroactive modifications of consciousness; and these psychical resultants are no less important than their physiological concomitants. Emotion is attended by a mental agitation, just as it is attended by a physical agitation. A multitude of ideas surge into consciousness and disturb the equilibrium. Some years ago, attention was called to a phenomenon which has since been referred to as the hypermnesia of the dying. Those who have escaped from imminent danger report that at the moment of impending death they saw before them as in a panorama, the chief events of their lives. This is simply a case of the phenomenon of hyper-ideation which characterizes many of the emotions. Dreams, muttering in somnambulistic states, hallucinations, and even indefinite interrogations are only an exaggerated form of the phenomena observed in the normal individual who talks to himself of an event which has made a violent appeal to his emotions.

Then too the emotions are characterized by feelings which are analogous to those already discussed—feelings of weariness and of powerlessness. The subject's personality undergoes a change; he no longer feels like himself, and even the external world loses its reality in greater or less degree.

Depression is no less a feature of emotion than is agitation. The depression may be visceral; it may manifest itself in a diminution of circulation or of respiration (which in emotion as in sleep, assume the intermittent type of Cheyne Stokes), in impairment of digestion and in gastro-intestinal debility. It may be motor, and evince itself in all the forms of weakness and paralysis which are found to attend certain emotions. A passage from Tolstoi which has been cited by Dumas in his book on '*La tristesse et la joie*,' furnishes an excellent illustration of this feature: "The assassins could easily have escaped from the scene of their crime, but they were so overcome by emotion, so enfeebled in all their limbs that they found themselves incapable of flight. Feeling wearied as though by a long walk, they lay

down upon the road, and there they remained until they were arrested." The mental depressions are particularly interesting. Popular observation noticed long ago that the individual, when overcome by emotion, is 'not himself,' that he is 'beside himself.' And I have shown on numerous occasions that the characteristics which have been acquired by education and moral development may suffer a complete change under the influence of emotion. People who have learned to speak correctly revert to dialect or resume a foreign accent when they are deeply moved. Their writing becomes confused, clumsy, boyish and full of faults; their whole character becomes coarse and debased. These general depressions are analogous to more definite disorders, and here again the disturbances of memory must be mentioned. Oblivescence of the event which occasioned the emotion, and inability to remember facts which immediately preceded, have frequently been found to accompany intensely emotional experiences in the form of continuous and retrogressive amnesia. But it must not be thought that these phenomena are merely pathological caprices. They are an exaggerated form of a general disturbance of memory which is characteristic of all emotions.

In his celebrated book on *Mind and Body*, Hack Tuke remarked that emotions frequently render the subject insensible, and he reported having seen subjects become blind and deaf as the result of violent emotion. I too have described many similar observations and they have now become a common-place. These disorders of sensibility and memory are analogous to certain disturbances of perception and attention; and the analogy holds alike whether the object of apprehension be one's self or whether it be the external world. As to will, there can be no doubt that it disappears in the depressive emotions and that the subject, when under intense emotion, is unable to decide what to do; indeed he even loses the power to act upon previous decisions. Hence one may well ask whether the mental commotion is not a more important characteristic of the emotion than is the visceral change. And one seems justified in regarding the consciousness of an emotional state as being something more than a mere counter-effect of peripheral disturbances.

These intellectual modifications, these losses of memory, these lacks of decision, these doubts, these failures to see reality as it is and to react upon it as one has previously learned to do, together with the feelings of depression which result from the changed mental conditions, constitute, in my opinion, the essential feature of emotion; and the sensations which arise as a 'back-stroke' from the peripheral disturbances is nothing more than a reinforcement, like the added tone in the chord.

It is a remarkable fact that certain emotions are attended by effects which are diametrically opposite to those just described. This second type of emotion may induce calmness, strengthen the visceral functions, arrest the useless mental agitation and replace it by an increased activity of attention and will. This improved condition of attention and will strengthens the tenacity of memory; it gives rise to valid representations of reality and to effective reactions upon one's environment. There are emotions which elevate as well as those which depress, emotions which heal as well as those which destroy. And here again as in fatigue and in repose, in sleep and in the waking state, we find a remarkable illustration of the oscillations of mind.

Let us now leave those phenomena which may be regarded as normal and examine the characteristics of mental diseases. Here we shall find phenomena of exactly the same sort as those which have already been discussed. Pathological psychology owes much to the study of hysteria. That hysteria is characterized by phenomena which are analogous to those which have been established in fatigue, in the sleeps, and in the emotions, is clearly evident from the different theories of hysteria which have been advocated. Certain investigators have insisted that hysteria is a purely emotional disturbance (the old theory of Briquet). Others have held that hysteria is a sleep-disease — a neurotic disturbance which is due to an excess of sleep (Sollier). Others again, find an analogy between hysteria and fatigue, and make the former the effect of an excessive degree of the latter (as Féré did in 1885). As for myself I am an out-and-out eclectic; I believe that hysteria is a disorder of emotion, of sleep and of fatigue, because all of them are at bottom exactly the

same thing. In hysteria one may observe the same intensely exaggerated agitations as are to be found in convulsions, crises, spasms, hallucinations, and in all other cases in which ideas develop automatically as a result of suggestion. One may observe the same feelings of weariness, of powerlessness, and of automatism. "I can see my arms and my legs moving, but it does not seem to be myself. I am a marionette and somebody is pulling the string." One may observe especially the same depressions and the same inefficiency of the higher cerebral functions. Permit me to recall my investigations upon aboulia, aprosexia, and amnesia in hystericals. If an act is even moderately novel, if a situation presents a problem to be solved, the hysterical remains inert and powerless. It is a remarkable fact that the disturbances of mental synthesis which occur in hysteria, bear a close resemblance to the oscillations of mind which have already been described. For example, suggestion which plays so important a rôle in this disease, can only be explained from the absence of antagonistic ideas which might counteract the idea suggested. This fact shows that the idea remains isolated in the mind of the hysterical, that it develops in the midst of a void, that the picture is not inclosed in a frame. And this is exactly what has been found to be characteristic of dreams. The anæsthesias, and frequently the paralyses, of the hysterical, alike consist of a reduction or narrowing of consciousness which is no longer able to make a simultaneous fusion of all the sensations and all the images which come in from without. This is well explained from the remarkable facts which relate to the transference, or better, the equivalence of phenomena in hystericals. One symptom gives place to another, one paralysis is cured and another supervenes, as though the mind were incapable of constituting a single system, and could resume control of one side of the body only at the expense of losing control of the other side. The narrowing of the field of consciousness in hysteria is of the very same sort as we found to occur in sleep and in fatigue.

In hysteria we again find interesting disturbances of memory — continuous and retrogressive amnesia — which are identical with those that occur in dreams and in emotion. It is a familiar

fact that at the end of an emotion we find that we have forgotten the preceding events, and we are incapable of acquiring the memory of new events. But hysteria is nothing more than this; and that is the reason why an endless discussion has arisen as to whether the hysterical is not merely an individual who has been overcome by emotion, and as to whether traumatic neurosis is not simply hysteria. The narrowing of the field of consciousness seems to me to be the characteristic form which the mental depression assumes in hysteria.

Let us now consider another disease which I have studied these many years, and which I have discussed in my most recent volumes. Let us take a glance at the innumerable disorders which have been designated obsessions, impulsions, insanity of doubt and of touch, tics, phobias, etc. No matter how various their symptoms may appear, it is possible to find certain fundamental characteristics which are common to all of these diseases. Motor, visceral or mental agitation manifests itself in unmistakable form in all of these crises of motor agitation, these contortions and tics of all sorts, and in the anguish which constitutes the essence of all the phobias. Everybody knows the peculiar mental agitation of those abnormal individuals who busy themselves incessantly with some insoluble problem, who spend whole days in an endeavor to remember what they did at a certain hour on a certain day ten years ago; who exhaust themselves in attempts to understand why trees are green, or why people have noses; who try to count all the objects they see, or to atone for every act by an appropriate exorcism.

All of these agitations seem to have their source in certain feelings which are extremely varied and interesting. I shall mention only the most familiar forms. In connection with all his acts the subject experiences feelings of difficulty, of inutility, of incapacity, of indecision, of uneasiness, of automatism, of domination, of discontent, of humility, of shame, of intimidation, and of revolt. In connection with his intellectual operations he has feelings of difficulty, of insufficiency, of instability, of imperfect perception, of gloom, of strangeness, of *jamais-vu*, of mal-orientation, of isolation, of mal-recognition, of *déjà-vu*, of presentment, of unreality, of dreaming, of the lapse of time, of

lack of intelligence, of obscurity, of doubt. In connection with his emotions there occur feelings of indifference, of weariness, of anxiety, of ambition, of need of excitement. In connection with his personality one may note feelings of self-estrangement, of double-personality, of depersonalization, of death, etc.

These feelings are far from being fictitious; they are based upon a real depression of the physiological functions. It is possible to establish real disorders of will which are manifested in indolence, irresolution, slowness, enfeebled effort, fatigue, failure of achievement, absence of resistance, misoneism, social aboulia with insurmountable timidity, professional aboulias and inertias of all sorts. One may establish disorders of intelligence which are manifested in amnesias, doubts, arrests of instruction, unintelligibility of perceptions, inattention, reveries, and veritable eclipses of mind. In connection with the emotions one may note indifference, melancholy, need of loving and of being loved, fear of isolation, and a return to childhood.

It is quite probable that depression phenomena similar to those just discussed, and that feelings of imperfection similar to those just summarized, are fundamental to many of the deliriums. In the delirium of persecution there are many phenomena of this sort, along with disturbances which are vaguely designated disorders of the general sensibility, and which characterize the first period of inquietude.

If these symptoms of depression — the motor retardation, the difficulty of apprehension and of association — become aggravated, one finds various forms of melancholia, whose interpretation constitutes an important problem of pathological psychology. Indeed, it is the chief problem as Kraepelin and his pupils have pointed out. Certain of these depressions are definitive and irreparable; they terminate more or less rapidly in one or other of the forms of dementia. Other depressions are transitory and curable. Is it possible to distinguish them, from the outset? That is, at the present time, one of the most important practical problems.

It is a remarkable fact that almost all of the depressions which we have discussed — hysteria, psychæsthenia, as well as melancholia — may under certain circumstances, disappear or

change into the opposite condition. We may designate this change as an excitation in order to distinguish it from the previous agitation which accompanied the depression. A hysterical subject may find herself changed as the result of a crisis, a somnambulism or a suggestion. "I am no longer the same," she says, "I feel new life. My head seems new." She is impressed by the fact that she perceives things much more distinctly than before. "I seem now to see the present objects for the first time. I saw them before, it is true, but they appeared to be in a distant fog. It is only now that I really recognize them." These feelings extend to other functions; it seems to her that she breathes more freely, that her arms and her legs are stronger but at the same time she has a much more intensive feeling of fatigue. The subject's conduct has undergone a complete change; she sets to work; she resumes her trade without ennui and even with interest. She is capable of making whatever coördinations are necessary, while in her previous condition she remained passive and inert for an indefinite period of time. Her sociability and her natural feelings return to full activity. I have elsewhere described changes of this sort in connection with the influence of hypnotism, and the necessity of direction in hystericals.[1]

It is to be noted that these changes appear in exactly the same form in psychæsthenic subjects, as the result of certain emotions, as the result of acts which they have been made to perform, or simply in consequence of exhortation or advice after their confidence has been won. Their disorders of perception, their doubts as to the reality of things and of themselves, disappear and are replaced by feelings of certainty which delight the subject beyond measure. He comes to know himself again, and he experiences deep feelings of emotion, of joy and sorrow, to which he had formerly been a comparative stranger. This change is accompanied in many cases by feelings of joy and delight which it is very important for us to know if we are to understand the mental states of certain religious ecstatics which science is only now beginning to analyze.

I can only indicate the most striking phenomena which I

[1] *Névroses et idées fixes*, 1898, Ch. XI.

have observed in connection with the influence of toxines in determining these periods of excitation. Of prime importance for the theory of these diseases is the fact that the fever induced by an intercurrent disease frequently suffices to cause the disappearance of depression and of all disorders depending upon it. I have called attention to certain curious cases in which the development of phthisis has brought about a cure of mental diseases. Women who have been subject to obsessions or agrophobia for twenty years without interruption, regain their calmness and moral assurance during the last months of their lives, when the progress of tuberculosis induces a slight degree of continuous hectic fever. But what I wish to emphasize here is the simple fact that all the symptoms of depression may disappear and give place to an opposite condition.

This is still more evident in the melancholic states of which we have just been speaking. It is known that melancholia may give place to a state of more or less normal excitation in which many of the preceding phenomena are reversed. This is what the older French alienists (Morel, Baillarger), and more recently Ritti, studied under the name of intermittent insanity, circular insanity, and insanity of dual form. This too is what the German alienists are taking up again under the name of depressive insanity, to which they rightly ascribe a great importance. The physiological and the mental conditions presented by these two contrary forms which alternate in the same individual, were carefully investigated by Dumas in his work on *La tristesse et la joie*. It would, in my opinion, be most desirable to analyze the states of mental excitation with the same care as has been given to the states of depression. It would be well to discover if the apparent exaltation of mind is real, to determine what pathological phenomena it manifests, and to ascertain whether it can, like depression, become the starting-point of delirium. It is at least certain that depressed subjects believe the ascending oscillation to be possible, that they desire it, and that they make every effort to attain it. Many of the impulsions are due to this fact. Dipsomania is in reality a crisis of depression in which the subject feels the need of being excited by means of a poison whose effects he knows only too well, *i. e.*, by alcohol. And there are many impulsions of the same sort.

How are we to envisage these transformations which are still far from being understood? It will suffice to summarize them in the form of a general hypothesis which may serve at once to resume a great many facts, to provoke discussion, and to instigate investigation. The phenomena which we have passed in review testify to the fact that the various functions of the nervous system are not all of equal difficulty. Certain functions are more facile than others, and require a lesser amount of nervous energy for the production of their mental phenomena. These functions seem to be arranged in a hierarchy of increasing degrees of difficulty; for when a nervous system loses or regains its strength, its functions disappear or reappear in a regular sequence. The functions which are the first to disappear are evidently the most complex, *i. e.*, those which are concerned with the synthesizing of a great number of sensations and images. We must therefore take account, as has been done particularly in England since the work of Hughlings Jackson, of the order of development of cerebral centers and cerebral functions. The functions which are the last to be developed in the race and in the individual are evidently the most complex and difficult; they will naturally be most readily affected in fatigue, in sleep, in emotion and in diseased conditions of the nervous system. Finally, I believe that these two notions may be united by the introduction of an additional conception. The mental operations which are at once most difficult of accomplishment and most recent in origin, are those whose function it is to bring the individual into relation with the given reality of the moment. They are most complex because reality is in touch with us at so many points, and most recent because the world about us is constantly changing. Evolution is not a thing of the past alone; we are constantly called upon to adapt ourselves to new situations, and to evolve new organs and new functions as our animal ancestors did in developing to our present condition. Now, one can readily see that it is just the adaptation to the present reality, the reaction upon reality, the feeling and enjoyment of reality, which disappears in all depressions, and which, on reappearing in the subsequent excitations, gives rise to feelings of joy and gladness.

Below these highest functions are to be placed those mental operations which occur when present reality is to a certain extent ignored, and the present reaction consists in an automatic repetition of the past. "I must not pay attention; my work will not proceed satisfactorily if I become absorbed in it." Still lower down, we must place the abstract mental operations; these have to do solely with few and non-complex images, nor are they concerned with new adaptations. It is a mistake to suppose that abstract reasoning, imagining and remembering are the highest mental operations. These are of value only when they are engaged upon the (concrete) present; so soon as they become abstract they cease to be difficult, and prove to be most common-place achievements. A high degree of development of purely representative memory is frequently attained by savages, by children, by the feeble-minded, and by the insane. Still lower down, we would have uncoördinated visceral excitation, such as is present in the emotions, in the uncoördinated motor agitations, in tics and in convulsions.

In short, the mental functions disappear more readily in proportion as their coefficient of reality is higher, and persist longer in proportion as their coefficient of reality is lower. Thus from the point of view of knowledge and of action, or of their correspondence with each other (Spencer), the mental functions constitute a series of decreasing difficulty, according as their relation to reality diminishes. If we consider these conceptions in connection with the philosophical views of Spencer, Höffding, Ribot, and Bergson, they throw light upon many of the observations and experiments of pathological psychology.

They permit us to take our stand upon a first group of facts relating to the lowering of the mental level in various conditions and diseases, and to interpret the changes which attend the various excitations, as a subsequent elevation of the mental level. In short they permit us to regard as mental oscillations, a host of phenomena which fail to find a place within the limits of a system of normal psychology. A psychology which confines itself to the study of normal mankind has a tendency to deal only with immutable and complete conditions. It is to a considerable degree static; it describes man as he ought to be, and

it does so in a definitive manner. It has a fondness for the functions which seem to be invariable, such as reason, representative memory, or the intellect in general. Such important phenomena as those of fatigue are not even mentioned in the older psychologies. Fatigue had no place in the older text-books because it was not regarded as a function; it was only a disorder. The older psychologists were equally at a loss in their discussion of sleep and dreams; these were regarded as mental curiosities and were relegated to an appendix at the end of the volume. And as to the emotions, they were misunderstood to the extent of being made phenomena either of intellect or of pure sensation. Pathological psychology has rehabilitated all these phenomena, and has pointed out their supreme importance. We cannot afford to ignore fatigue, sleep and emotion, since they are typical of all the mental diseases. If we ignore them we must also ignore hysteria, obsessions, melancholia and mania. And what would be left? In my opinion, it was psycho-pathological investigation which introduced into psychology the disposition to pay more attention to the evolution of mind as manifested in the phenomena of mental augmentation and mental diminution. In a word, psycho-pathology has made psychology more dynamic, and it seems to me, has also deepened it, and brought it closer to reality.

This rapid sketch shows us what has been the direction taken by the chief investigations of pathological psychology. We have summarized the results of numerous investigations which have already been made, and have indicated the trend of those that are to come. What problems are set for us to solve by the notion of an oscillation of the mental level? What phenomena are characteristic of the depression and of the reëlevation (excitation) of the level? In other words, what precise position in the hierarchy is occupied by each mental function? A rapid association of ideas, and a development of automatism do not always indicate an elevation. There are agitations which coincide with depressions, and which may be regarded as a sort of derivative. How does the derivation come about? How do the phenomena belonging to a lower level replace a vanished phenomenon of a higher level? What are the characteristics

of excitation, which has been studied much less than depression? What factors determine these two groups of phenomena? How does it come that in different diseases these phenomena appear now in one form, now in the alternate form? What is the mental result of the indefinite prolongation of a state of depression or of excitation? The answers to these questions will doubtless some day help us to solve the difficult problem of the classification of mental diseases. Finally, is it possible to discover therapeutic agents, whether physical or mental, which will act upon the oscillations? Our knowledge upon all of these points is still in a rudimentary stage. But it seems safe to assert that the notion of the elevation of mental levels is beginning to assume a definite form ; and that it has opened up to us an important chapter of pathological and of normal psychology.

SOME OF THE PRESENT PROBLEMS OF ABNORMAL PSYCHOLOGY.[1]

BY PROFESSOR MORTON PRINCE,

Tufts College Medical School.

I.

To discuss the present problems of abnormal psychology without acknowledging the debt we owe to the distinguished psychologist who has addressed us to-day would be as impossible as it would be ungrateful. One may be permitted to question whether a section on abnormal psychology would have appeared upon the programme of this congress if Dr. Pierre Janet had not already gone before and opened up this great field of investigation through his brilliant researches. It is not too much to say that as numerous as are the problems awaiting solution, there is scarcely one which has not already been illumined by this investigator's penetrating observations.

In our own country, too, we owe much to Boris Sidis, a patient student and keen investigator of psychological problems, whose researches in the dissociation of consciousness and genesis of hallucinations have given precision to our conceptions of these abnormal conditions. The time at my disposal will not allow me to refer by name to the work of other students, though I cannot forbear calling attention to the great impetus given to the study of this fascinating field of research by the labors of Charcot and of the brilliant Salpêtrière group of scholars who still love to call their old Chief master. Certain problems in subconscious automatism will always be associated with the names of Breuer and Freud in Germany, and Alfred Binet in France. It is encouraging to see the growing interest in this field, and the increasing number of students who are pursuing its problems.

[1] An address delivered before the Section on Abnormal Psychology at the St. Louis Congress of Arts and Science, September 24, 1904.

As a field of research abnormal psychology belongs both to the psychologist and the physician. It has thrown much light on the mechanism of normal mental processes, for disease dissects the mind and brings into view the mechanism of its processes much better than can the introspective study of the psychologist. In the department of medicine it has furnished an intelligible explanation of many previously incomprehensible derangements of the mind and body. With this increase in precision of our knowledge of mental processes it has ceased to be sufficient for the physician to know that an anæsthesia or paralysis or other disturbed function of the body is due to some mysterious mental influence, but medical culture requires that he should know the exact mechanism of this influence. The researches of recent years have furnished this knowledge in many important particulars.

Dissociation and Automatism.

Abnormal psychological phenomena, as phenomena, may be divided into two great groups, according as they are manifestations of (A) dissociations or weakened syntheses of conscious states, or (B) of automatism.

In the first group (A), the dissociations and imperfect syntheses, may be placed the losses of memory (amnesias), the losses of perceptions (anesthesias), the losses of motor functions (paralyses), the alterations of character, the divisions of personality, etc.

The .second group (B) the automatisms, would include all those phenomena which are the expressions of an activity beyond the will and control of the personal consciousness and involve abnormal syntheses. It would include the fixed ideas, the hallucinations, the deliriums, the obsessions, impulsions, tics, contractures, convulsive seizures and various perversions of the visceral processes. The automatism may be still further classified according as they are syntheses of dissociated (so-called subconscious) elements, so characteristic of hysteria, or syntheses of the personal consciousness, characteristic of psychasthenic states. Both may exist together.

These two classes of phenomena (A and B) bear a reciprocal

relation to one another, in that pari passu with the development of a weakening of the power of synthesis, or of a complete dissociation, the remaining restricted elements of the personal consciousness, or the dissociated elements, respectively tend to take on automatic activity; as an example, take the obsessions of psychasthenia confined entirely to the personal consciousness, and the hysterical attack due to the automatic activity of dissociated (subconscious) memories of past experiences. And *vice versa*, the development of automatism with its abnormal syntheses tends to induce dissociation, as when an artificially induced idea robs the personal consciousness of its sensory perceptions (anesthesia) or produces retrograde amnesia. Thus in any particular syndrome, such as the hysterical state, or the psychasthenic obsessions, we have combined the manifestations of dissociation or weakened synthesis with those of automatism.

The problems of abnormal psychology become, then, very largely problems of dissociation, weakened syntheses and automatism; and if the laws which govern these processes can be determined, we shall be able to correlate most, if not all, abnormal psychological states. As dissociation and automatism are also principles of normal mental life, as for example, the phenomena of absentmindedness and the artifacts of suggestion, in these same laws we may find a correlation of abnormal psychology with normal psychology. Thus simply as manifestations of, or perverted working of, the normal dissociating and synthesizing process, whether psychical or cerebral, we may find an explanation for and correlate:

1. Physiological states, like
   Sleep,
   Dreams,
   Normal forgetfulness (amnesia),
   Moods,
   Absentminded phenomena,
   Natural somnambulism;

2. Artifacts like,
   Hypnosis and post-hypnotic phenomena,

Motor automatisms; (automatic writing and speech),
Artificial sensory automatisms (crystal visions);

3. Pathological states; such as,
The hysterical state and its manifestations,
Trances,
Obsessions,
Hallucinations,
Deliriums,
Alterations of character,
Multiple personalities,
Epileptoid states, etc.

I think we may also, though perhaps debatably, include certain insane states, like moral and certain forms of circular insanity. It is certain that abnormal phenomena like retrograde amnesia, hysterical anesthesia, hallucinations, alterations of character, etc., do not stand apart as facts without kinship to normal life, for phenomena, which are absolutely identical in form, content and behavior to the environment, whether spontaneous or artificially induced, occur as manifestations of the activity of the health mind. Thus certain types of normal amnesia may in every way give the same reactions, and show the same relations to the personal consciousness as abnormal amnesia; normal absentmindedness as abnormal abstraction; crystal visions as hysterical visions; normal or induced somnambulism as hysterical somnambulism; post-hypnotic suggestion and fixed ideas, automatic writing, and the dowsing-rod as the motor automatisms and fixed ideas of disease. The difference between normal and abnormal dissociated states probably depends upon differences in the lines of disaggregation, psychical and physiological, and not upon the differences of process.

If Virchow's great generalization is true, namely that disease is only life under altered conditions, we may say that the phenomena of abnormal psychology are only the normal processes of the mind and brain submitted to changed conditions. One great vantage point of abnormal psychology is that by altering the conditions at will, as we often can do, we can study

the alterations in the normal processes and thus find out what those processes are.

It has been, indeed, through a study of the abnormal, that is, a study of natural forces under altered conditions, that the physical sciences have received their development. It was by such a study of abnormal phenomena that Galileo was able to demonstrate the laws of inertia and of falling bodies; that Archimedes proved the theory of his lever, and that Pascal demonstrated his hydro-dynamic paradox. In fact, all physical research depends upon the study of abnormal phenomena.

The Mind not a Unity.

One of the great truths taught by abnormal psychology is that while the mind under ordinary conditions is for practical purposes a unity, under altered conditions it may cease to be a unity, and may exhibit multiple activity of a complex sort. It is even questionable whether under habitual conditions it is ever an absolute unity, whether *within certain limitations* it does not always exhibit a certain degree of multiplicity. It remains a problem, which I have thought well worthy of special consideration at this time, to investigate what those limitations are.

Influence of the Mind on the Body.

Again, considered from the two points of view of dissociation and automatism, we are able to approach those puzzling problems which belong to practical medicine and which have long baffled clinical research. From the earliest times to these days of scientific skepticism about the veridity of phenomena which cannot be explained, the influence of the mind on the body for ill or for good has been and is recognized. Its influence for evil is evident in the nervous manifestations which mimic organic disease, in the perverted functioning of the organs of the body, and even in its deleterious effect upon organic pathological processes. Its influence for good, in the dissipation of these same manifestations and perversions through faith cures, fads, modern therapeutic suggestion in its various forms and mental hygiene. The lack of precise knowledge of the psychology of these states and of the modus operandi of

the therapeutic agency employed has given rise to all sorts of pseudo-scientific therapeutic systems (including the misuse of drugs), and to the growth of dogmas, philosophies and religions. The hopeless muddling of even the educated medical mind in this field of abnormal psychology is made manifest by a casual perusal of the standard text-books on medicine as well as the latest monographs on mental therapeutics. In the laws of dissociation, weakened synthesis, and automatism, abnormal psychology offers a basis upon which to support an intelligible explanation of these perplexing phenomena — slight as is our knowledge of the details of these processes. Thus in the automatic activity of subconscious fixed ideas we have a demonstrated casual factor in many so-called hysterical phenomena (attacks, tics, anesthesias, etc.); and in the nervous radiations from these subconscious automatisms down through the lower nervous centers we find the origin of various disturbances of the body, as when subconscious, or partially subconscious, emotional states excite cardiac, vaso-motor, secretory and other visceral derangements.

Hysteria.

The study of abnormal psychology has completely revolutionized our conception of that remarkable disease, hysteria, as much so as the discovery of germs has altered the surgical conception of inflammation. We understand to-day that it is not only a disease of the mind, but more precisely that fundamentally it is a splitting of the personality with or without a doubling of consciousness and automatism. But we are still however entirely in the dark regarding the modus operandi of many of its manifestations. The exact mechanism for instance, by which such phenomena as contractures, epileptiform seizures, and vaso-motor disturbances are brought about, though recognized as dissociated automatism, remains as a problem for the future.

Character.

Another problem which must be approached along these same lines are the modifications of character which occur in diseased conditions like hysteria, particularly that special type

known as disintegrated multiple personality, and in certain psychoses like circular and so-called moral insanity. The lesser states of depression and exaltation as commonly observed in such conditions each accompanied or represented by altered points of view of the individual when closely studied seem not to essentially differ from the similar conditions experienced by normal individuals and called *moods*. These alterations of character can be studied most advantageously in disintegrated personality where they can be watched as they take place under the very eye of the observer. They may include even changes in the physiological reactions, like the effect of alcohol, tobacco, sunlight and the environment, and may or may not be accompanied by alterations of memory. Thus to take an actually observed example, an individual at one one moment amiable, religious, forbearing, the typical saint of literature, one who loves the quiet idealism and subdued light of the cloister and whose moral and physiological tastes lead her to dislike coffee, cigarettes, wine, the glare of sunlight, gaudy fashions of dress and a score of other pleasures of the flesh, becomes suddenly, in a moment of time, strong, resolute, 'quick and sudden in quarrel,' without religious tastes, one who delights in cigarettes, wine, the pleasure of the table, gay fashions of dress and above all in the strenuous Rooseveltian life. More than this the character of an individual of this instability can be modified almost at will by artificial methods, or indirectly through the effect of fatigue and emotion. A certain number of the traits of one state being swapped for those of another and *vice versa*.

Similar alterations of character have been artifically brought about by one experimenter in an individual with an apparently healthy nervous system, five different character states being obtained without changes of memory.

Such phenomena as these raise the problem of what is character, or more specifically what is the fundamental process which brings about the alterations observed in special diseased conditions? The data at our disposal do not allow us to completely answer these questions, but we have enough facts at hand to show that a very large share in the process must be attributed to either psychical dissociation, or incapacity to make

complete syntheses, in consequence of which the 'personal perceptions' are very much restricted.

The fact that the same sort of alterations occur in the profound psychoses, in hysteria, and even in the moods of normal life suggest that all are different types of disaggregation of the field of consciousness and perversions of the same mechanism, though the exciting cause may be different in each case.

The problems of functional dissociation, abnormal syntheses and automatisms belong to those which are fundamental to abnormal and therefore normal psychology. We enter upon more debatable ground when we try to determine the exact nature of these processes and give greater precision to our knowledge. Are the anesthesias, amnesias and other forms of dissociation, for instance, to be classified as held by Dr. Janet as 'failure of personal perception,' that is, simply as special types of normal absentmindedness, or must some physiological principle be invoked. This is a question of interpretation of the observed facts, and it seems to me, is one which we are not in a position as yet to answer definitely, although as I shall later point out we can frame a reasonable hypothesis.

II. Do Subconscious States Habitually Exist Normally, or are They Always Either Artifacts or Abnormal Phenomena?

I have already referred to the doubling of the mind and the formation of subconscious states that may result from this dissociation, even to the formation of a second personality. Now, if abnormal dissociation is only an exaggeration, or perversion of normal dissociation, the question arises, To what extent is there a division of the healthy mind, of such a character as to give it multiplicity? Are the well-known abnormal dissociations and automatisms, the manifestations of abnormal subconscious processes, merely perverted types of similar processes which go on in every healthy mind? This is one of the most pressing problems for abnormal psychology to settle, for the idea that there is a subconscious mental life of elaborate activity and which habitually plays a large part in all our mental processes has received such wide acceptance that it shows evidence of

dominating psychological thought and has even furnished a ground work for a new philosophy. As a problem in dissociation and automatism I propose, therefore, to inquire to what degree this hypothesis is justified by actually demonstrated data in our possession to-day.

The problem may be thus stated: Do subconscious states habitually exist normally, or are they always either artifacts or abnormal phenomena? If they form a part of the normal mind what is the extent of the subconscious field? There is a very wide tendency at the present day to account for a large variety of phenomena, including both normal and abnormal experiences, by what used to be called 'unconscious cerebration' but which now is spoken of as 'subconscious thought' or the 'secondary consciousness.'

Now at the outset, in approaching this problem, we should have a clear idea of what is meant by subconscious ideas and their relation to the personal consciousness. It is difficult to state the theory of a secondary consciousness in a way that will be acceptable to all students, for probably no two observers are agreed as to the interpretation of the facts, or, if the fundamental notion be accepted, whether the theory includes a limited or a large category of facts. All, however, are agreed that, *under certain artificially induced or abnormal conditions*, correlated with our brain processes at any moment of time there may be a certain number of elementary conscious states of whose existence we are ignorant, but which nevertheless coëxist with that habitual waking consciousness which we term ourself, or our own personality.

Now as to the *conditions* under which this secondary consciousness develops, and to its *extent* — the number of sensations, emotions and other psychical states composing it, and above all, the degree to which they are organized into a self-acting system (or personality) — there is considerable difference of opinion, so that there may be said to be several theories of the secondary consciousness, according to the point of view of the writer, and the interpretation given to the accepted facts. While all agree that under *special* conditions every mind may be made up of certain states of which we are conscious and

certain states of which we are not conscious, some think that in *healthy* minds the secondary consciousness — if existent at all — is limited to only a number of more or less dissociated and isolated states, like sensations and perhaps emotions, without being synthesized into a personal self unity, or even self-acting system. Others think that these dissociated states are always woven into a systematized unity and are capable of considerable intellectual and independent activity. Some think that these secondary states play but a small subordinate part in our mental lives; others think that they have a very large share in our daily acts, particularly in those acts to which we do not give our conscious volition (habit acts, absent-minded acts, etc.). Still others seek to explain our highest intellectual feats through this secondary consciousness. It will be borne in mind that we are now speaking of normal healthy minds. In diseased minds it is agreed by all that the psychical states making up this secondary consciousness may become highly organized into a self-acting system and become capable of playing a rôle almost as controlling and independent as the habitual self. But some (Janet) think a doubling of consciousness is always a sign of disease.

Now subconscious ideas are dissociated ideas — dissociated from the main system of ideas which make up the personal consciousness. They are thrown off, so to speak, as satellites may be supposed to be thrown off from their planet. The term 'subconscious' is an unfortunate one for it is metaphorical, and, while descriptive does not precisely express the true relation of these ideas to the personal consciousness. Extra-conscious, concomitant or better, dissociated, are more exact terms. Now being dissociated from our personal consciousness, we are ignorant of them. Our knowledge of the existence of such dissociated mental states is largely derived from a study of pathological and artificially induced conditions, where their presence can be positively and accurately determined. The researches of recent years have proved very conclusively not only that the mind may be split in two in such a way that certain groups of ideas may be dissociated from the main consciousness, but that a number of these dissociated states may become synthesized

among themselves and that in this way is formed a second consciousness, capable of a certain amout of activity. This activity may be manifested contemporaneously with that of our personal consciousness. There is then a doubling of consciousness. The mind becomes dual. Thus in the subject of disintegrated personality just referred to, known as The Misses Beauchamp, a secondary group of dissociated states has existed for many years contemporaneously with the personal consciousness. These secondary states are so extensive and are so well organized into a personality that I have been able to obtain an autobiography of the subconscious life of this concomitant personality, disclosing a mental life running along side by side with, but unknown to, the personal self from childhood to the present day. The subject is twenty-eight years of age. Similar though less extensive manifestations of a double life are common as phenomena of hysteria. In the automatic writing and speech of mediums and of psychological experiment, in the dowsing rod, in so-called post-hypnotic phenomena, and in the automatic acts of artificial and spontaneous abstraction, we have the same manifestations of the splitting of the mind and the formation of an extra-conscious self of which the personal consciousness is ignorant. The dissociated states may or may not take on contemporaneous activity. If they do so the secondary phenomena thus produced are called *automatisms* as they occur outside the cognition of the personal self. They form the subconscious fixed ideas of hysteria now so well known. When the dissociated ideas include the kinesthetic and sensory spheres we have hysterical paralyses and anesthesias. At times these dissociated ideas break out in insurrections, kick up didos and turn our peaceful mental arrangements topsy-turvy. We then have the hysterical attack.

Now allowing for such differences of opinion as have been already stated there still seems to be a tacit acquiescence on the part of many psychologists in the theory that in normal healthy minds similar dissociated ideas of greater or less complexity have their place and play a well-regulated part in the mental economy. In other words, according to this theory the normal mind is not a unity any more than the hysterical mind. It re-

quires but a slight extension of this theory to assume as some do that these dissociated mental states become normally synthesized into a second consciousness of considerable intellectual capacity, which takes part in our every-day intellectual processes. In every mind the activity of the primary consciousness is supposed to be accompanied by that of a secondary consciousness. On the basis of actually substantiated data one would think that this was as far as the hypothesis could be logically carried, but the fact that we are not conscious of dissociated ideas gives a certain mysticism to their existence and has offered a temptation to still further extend the hypothesis until in the hands of certain of its advocates it has outgrown even all demonstrated pathological facts. The subconscious ideas, instead of being mental states dissociated from the main personality, now become the main reservoir of consciousness and the personal consciousness becomes a subordinate stream flowing out of this great storage basin of 'subliminal' ideas as they are called. We have within us a great tank of consciousness but we are conscious of only a small portion of its contents. In other words, of the sum total of conscious states within us only a small portion forms the personal consciousness. The personal self becomes even an inferior consciousness emerging out of a superior subliminal consciousness present in a transcendental world, and this subliminal consciousness is made the source of flights of genius on the one hand, while it controls the physical processes of the body on the other. It is hardly necessary to follow this new 'tank' hypothesis into its different applications. I merely refer to it as it has unquestionably colored the orthodox conception of subconscious ideas. Thus Professor Stout,[1] while contending against this doctrine, himself apparently influenced by it, postulates normal dissociated states (he adopts the term 'subliminal') and gives them functions of wide scope.

"Consider," he says, "the process of recollecting a name. * * * It may happen that we fail to revive the name while we are trying to do so, and that it suddenly emerges into consciousness after an interval during which we have been occupied with other matters, or have been asleep. This implies that our conscious effort has set going a subliminal process which continues after the conscious effort has ceased."

[1] *The Hibbert Journal*, October, 1903.

Professor Stout then goes on to argue that our conscious process has a way of exciting these dissociated states into trains of thought of which we are wholly unconscious and which solve our problems for us while we attend to other things.

"In such cases" [solving problems], he says, "conscious endeavor to find an ideal combination which shall satisfy certain conditions serves only to set in operation subliminal processes which may or may not yield the requisite result. Here also the process may continue after the consciousness which prompted it ceased. The ordinary man, no less than the man of genius, may find that what relatively to *him* are original ideas develop while his thoughts are occupied with disconnected topics, or even while he is asleep. In general we take an utterly false view of mental construction when we regard it is a mere putting together of data already present in consciousness analagous to the putting together of the parts of a puzzle spread out on the table before us."

It seems to me that these are pure assumptions. As far as my own conscious experience goes I am compelled to agree with Mr. Andrew Lang[1] in that as 'an ordinary man' I do not find that my conscious activity appeals to 'anything else' but my own conscious processes, or that I am conscious of any such easy way of settling my own problems. As an ordinary man I do not find I can rely upon any other consciousness to write this address but the thoughts which I laboriously elaborate.

This theory of the normal occurrence of subconscious dissociated thought seems to have arisen as an interpretation of certain well-known, but exceptional, spontaneous experiences of the kind which Professor Stout accepts as evidence of normal subconscious mental activity, but the theory has a more substantial basis in data which have been obtained through direct objective experimentation. These include (1) various hysterical phenomena, (2) hypnotic experiments, (3) various motor automatisms, particularly automatic writing, and (4) phenomena of absentmindedness or abstraction. A critical analysis of these data will show that they do not permit of inferences applicable to normal and habitual conditions.

1. That secondary subconscious states, capable of being synthesized into a self, may be developed by disease is a well-attested observation. But, being pathological, they are evidence only of the abnormality of subconscious states.

[1] *The Hibbert Journal*, April, 1904.

2. As to hypnotic states, it is sometimes assumed that the hypnotic self represents a persistent consciousness having a continuous existence after the awakening of the personal consciousness. There is no evidence for this. The hypnotic self is a dissociated state of the waking consciousness. On awaking the synthesis of the original self is again made and the hypnotic dissociation ceases to exist. Nor is there any particular hypnotic state. There may be almost any number of such states in the same individual — as many as there are possible states of dissociation. In the second place, hypnosis is an artifact — an artificial dissociation, not a state of normal life. The phenomena of post-hypnotic suggestion, which are entirely phenomena of subconscious processes, are likewise artifacts, produced by the methods of the experiment. They prove that the mind may be artificially made to exhibit duality but not that this is true of normal mental life.

3. As to the evidence from automatic writing and similar phenomena, it seems to have been overlooked that these phenomena too are artifacts. Although they are plainly manifestations of dissociation of consciousness and automatism of the dissociated elements, nevertheless this dissociation is the product of the conditions of the experiment. Abstraction, which means dissociation of a greater or less degree, is induced and suggestion directly excites the phenomena. But all such experiments have great significance in another respect. The ease with which the mind, in perfectly healthy persons, can be dissociated and the dissociated states synthesized into an autonomous system shows that subconscious synthesized states are not always evidence of disease, as maintained by Janet, though they may be artifacts, but that the whole is dependent upon a physiological process. When a physiological stimulus, like the mere sound of a spoken word, a suggested idea, is capable of inciting a dual activity of the mind in healthy university students the process is unintelligible, unless it is psycho-physiological, that is to say, a normal reaction of the mind to specially devised stimuli. When critically examined, then, the experimental evidence, which is relied upon to establish subconscious ideas as normal processes of mentation, is found to be fallacious.

The resulting phenomena are made subconscious by the very conditions of the experiment. For this reason the problem is impossible to solve by the usual experimental methods. There is, however, some experimental evidence of a different sort which may be utilized, and which I propose presently to point out.

4. The phenomena of absentmindedness, or abstraction, a normal function, indicate both dissociation and automatism. It is not difficult to demonstrate experimentally that auditory, visual, tactile and other images, which are not perceived by the personal consciousness during this state may be perceived subconsciously. Thus, under proper precautions, I place various objects where they will be within the peripheral field of vision of a suitable subject, C. B. Her attention is strongly attracted listening to a discourse. The objects are not perceived. She is now hypnotized and in hypnosis describes accurately the objects, thus showing that they were subconsciously recognized. It is the same for auditory perceptions of passing carriages, voices, etc. Likewise, on the *motor* side the numerous absentminded acts of which we are not conscious show intelligent subconscious automatism. C. B., in hypnosis, remembers each step of such an act (putting away a book in the book case), of which she is completely oblivious when awake. This duality of the mind in normal absentmindedness has been pointed out by various observers. Its phenomena simulate those of artificial abstraction as they occur in automatic writing and hysterical states. There is nothing surprising in this as the term "absentmindedness" means dissociation of consciousness — a failure to perceive that which before was perceived, and a failure to be conscious of acts intelligently performed. On the other hand, normal absentmindedness is a distinctly special condition. We don't go about in an absentminded state, or as if we had lost our heads, when we have work to be done. Absentminded phenomena are manifestations of the temporary disintegration of the personal self, and doubling of consciousness, but not evidence of the persistence during the ordinary waking life of subconscious states. It does not follow that on waking from revery complete synthesis does not take place. But here the significant fact, the most significant of all, should

not be lost sight of, that in the normal process of abstraction we find evidence of the existence of a normal prearranged mechanism for dissociating consciousness and producing subconscious states. Dissociation is plainly a function of the mind or brain.

Now, the nub of the problem is, in healthy persons are these subconscious states limited to absentmindedness? and, if not, what part do they play in the mental economy? Indeed, whether so limited or not, what is their extent? *i. e.* (*a*) Are they purely isolated phenomena, isolated sensations and perceptions? or (*b*) Are they synthesized, as imagined by Professor Stout into logical subconscious processes of thought, capable of sustained action, and as imagined by some sufficiently complex to form a personality — something that we are justified in calling a subconscious self? or (*c*) Are subconscious states when synthesized always either artifacts or pathological?

The question is at the root of many important problems in abnormal psychology, but is difficult to answer by experimental methods, owing to the danger of artifacts. In illustration of this danger I may point to the phenomena of subconscious solution of arithmetical problems which are sometimes cited in evidence. In favorable subjects, as in an instance under my own observation, it is not difficult by means of suggestion in hypnosis to obtain the solution of arithmetical problems during the waking state by some other consciousness than that of the waking personality. For example, while in hypnosis, two numbers are given to be added or multiplied, say 453 + 367, or 4326 × 3, to take actual examples, and the subject waked instantaneously the moment the last figure is given. The addition or multiplication is correctly solved subconsciously, the subject not having any conscious knowledge that any task whatever has been set. The exact method of mentation by which the problem is extraconsciously solved is learned by catechizing the hypnotic personality. But such experiments are plainly artifacts. The dissociation and automatism are the products of suggestion. The results are of value, however, as cannot be too often insisted upon, in that they show the ease with which duality of the mind may be effected by what is plainly a psycho-physiological stimulus, a suggested idea. But to obtain subconscious phe-

nomena free from artifice such phenomena must be *spontaneous*. Information regarding the presence and character of subconscious states at any given time can be easily obtained owing to the well-known fact that ideas[1] dissociated from the personal consciousness awake may become synthesized with this same consciousness in hypnosis and then be remembered. A person in hypnosis may thus be able to analyze and describe the ideas which were spontaneously present as an extraconsciousness when awake, but which were not then known to the personal consciousness. This method is far more accurate than the device of tapping the subconsciousness by automatic writing though the same in principle. I am obliged here to refer to a series of observations of this kind which I have personally made with a view of obtaining light upon this question, as I know of no others that have been limited to spontaneous phenomena and are not open to the objection of artifacts. A systematic examination[2] was made of the personal consciousness in hypnosis,

[1] This word is used as a convenient expression for any state of consciousness.

[2] I have adopted this custom of treating the hypnotic self as a sane consciousness, instead of a freak affair, fit only to be played with and to be made to perform all sorts of antics. I am certain this method of study will throw more light on the composition of normal consciousness than that of inducing hallucinations and other artifacts. The hypnotic self if treated like a reasonable being, will be found able to give important information. It knows the waking self, it knows its own thoughts, and it knows the thoughts of the secondary consciousness. It can give very valuable information about each. On the other hand, it is very easily disintegrated by suggestion ; and ideas, hallucinations, and what not, are very easily created in it. Experiments of this latter kind have their use, but for the purpose of learning the mode of the working of the normal mind a still greater advantage is to be obtained by treating it as a rational consciousness, capable of accurately observing and imparting information derived from its own experiences.

I would here insist that it is a mistake to confuse the personal consciousness in hypnosis with the secondary consciousness when such exists. They are not identical or coextensive. A hypnotic self, as ordinarily observed, is still the personal consciousness, but in hypnosis the previously dissociated states are synthesized with this self and remembered. The whole becomes then a unity, and the hypnotic personal consciousness remembers the formerly dissociated ideas and its own and speaks of them as such. This has given rise to the wrong interpretation that identifies the hypnotic self with the secondary or subconsciousness. But the hypnotic self includes a large part of the waking personal self. On waking, this part regains the rest of its own syntheses and loses the second states. A failurc to recognize these facts has led to much confusion in interpreting abnormal psychological phenomena.

regarding the perceptions and content of the secondary consciousness during definite moments, of which the events were prearranged or otherwise known, the subject not being in absentmindedness. It is not within the scope of an address of this sort to give the details of these observations, but in this connection I may state briefly a summary of the evidence, reserving the complete observations for future publication. It was found that —

1. A large number of perceptions — visual, auditory, tactile and thermal images, and sometimes emotional states — occurred outside of the personal consciousness and therefore the subject was not conscious of them when awake. The visual images were particularly those of the peripheral vision, such as the extra-conscious perception of a person in the street, who was not recognized by the personal waking consciousness; or the perception of objects intentionally placed in the field of peripheral vision and not perceived by the subject, whose attention was held in conversation. Auditory images of passing carriages, of voices, footsteps, etc., thermal images of heat and cold from the body, were similarly found to exist extra-consciously, and to be entirely unknown to the personal waking consciousness.

2. As to the content of the concomitant (dissociated) ideas, it appeared by the testimony of the hypnotic self, that as compared with those of the waking consciousness the secondary ideas were quite limited. They were, as is always the experience of the subject, made up for the most part of emotions (*e. g.*, annoyances), and sensations (visual, auditory and tactile images of a room, of particular persons, people's voices, etc.). They were not combined into a logical proposition, though in using words to describe them it is necessary to so combine them and therefore give them a rather artificial character as 'thoughts.' It is questionable whether the word 'thoughts' may be used to describe mental states of this kind, and the word was used by the hypnotic self subject to this qualification. Commonly, I should infer, a succession of such 'thoughts' may arise, but each is for the most part limited to isolated emotions and sensorial images and lacks the complexity and synthesis of the waking mentation.

3. The memories, emotions and perceptions of which the subject is not conscious when awake are remembered in hypnosis and described. The thoughts of which the subject is conscious when awake are those which are concentrated on what she is doing. The others, of which she is not conscious, are sort of side thoughts. These are not logically connected among themselves, are weak, and have little influence on the personal (chief) train of thought. Now although when awake the subject is conscious of some thoughts and not of others, both kinds keep running into one another and therefore the conscious and the subconscious are constantly uniting, disuniting and interchanging. *There is no hard and fast line between the conscious and the subconscious, for at times what belongs to one passes into the other, and vice versa.* The waking self is varying the grouping of its thoughts all the time in such a way as to be continually including and excluding the subconscious thoughts. The personal pronoun 'I,' or, when spoken to, 'you,' applied equally to her waking self and to her hypnotic self, *but these terms were not applicable to her unconscious thoughts, which were not self-conscious.* For convenience of terminology it was agreed to arbitrarily call the thoughts of which the subject is conscious when awake the *waking consciousness*, and the thoughts of which when awake she is not conscious the *secondary consciousness.* In making this division the hypnotic self insisted most positively on one distinction, namely, that the secondary consciousness was in no sense a *personality.* The pronoun *I* could not be applied to it. In speaking of the thoughts of this second group of mental states alone, she could not say 'I felt this,' 'I saw that.' These thoughts were better described as, for the most part, unconnected, discrete sensations, impressions and emotions, and were not synthesized into a personality. They were not therefore self-conscious. When the waking self was hypnotized, the resulting hypnotic self acquired the subconscious perceptions of the second consciousness, she then could say '*I*,' and the hypnotic '*I*' included what were formerly 'subconscious' perceptions. In speaking of the secondary personality by itself, then, it is to be understood that self-consciousness and personality are always ex-

cluded. This testimony was verified by test instances of subconscious perception of visual and auditory images of experiences occurring in my presence.

4. Part played by the secondary consciousness in (*a*) normal mentation. The hypnotic self testified that the thoughts of the secondary consciousness do not form a logical chain. They do not have volition. They are entirely passive and have no direct control over the subject's voluntary actions.

(*b*) Part played by the secondary consciousness in absentmindedness. (1) Some apparently absentminded acts are only examples of amnesia. There is no doubling of consciousness at the time. It is a sort of continuous amnesia brought about by lack of attention. (2) In true absentmindedness there does occur a division of consciousness along lines which allow a large field to, and relatively wide synthesis of the dissociated states. The personal consciousness is proportionately restricted. The subconscious thoughts may involve a certain amount of volition and judgment, as when the subject subconsciously took a book from the table, carried it to the bookcase, started to place it on the shelf, found that particular location unsuitable, arranged a place on another shelf where the book was finally placed. No evidence, however, was obtained to show that the dissociated consciousness is capable of wider and more original synthesis than is involved in adapting habitual acts to the circumstances of the moment.

(*c*) Solving problems by the secondary consciousness. So much is to be found in the literature about subconscious solutions of problems that the following testimony of the hypnotic personality is of interest:

"When a problem on which my waking self is engaged remains unsettled, it is still kept in mind by the secondary consciousness, even though put aside by my waking self. My secondary consciousness often helps me to solve problems which my waking consciousness has found difficulty in doing. But it is not my secondary consciousness that accomplishes the final solution itself, but it helps in the following way. Suppose, for instance, I am trying to translate a difficult passage in Virgil. I work at it for some time and am puzzled. Finally, unable to do it, I put it aside, leaving it unsolved. I decide that it is not worth bothering about and so put it out of my mind. But it is a mistake to say that you put it *out* of your mind. What you do is, you put it *into* your mind: that is to say, you don't put it out of your mind if the problem remains unsolved

and unsettled. By putting it *into* your mind I mean that, although the waking consciousness may have put it aside, the problem still remains in the secondary consciousness. In the example I used, the memory of the passage from Virgil would be retained persistently by my secondary consciousness. Then from time to time a whole lot of fragmentary memories and thoughts connected with the passage would arise in this consciousness. Some of these thoughts, perhaps, would be memories of the rules of grammar, or different meanings of words in the passage, in fact, anything I had read, or thought, or experienced in connection with the problem. These would not be logical connected thoughts, and they would not solve the problem. My secondary consciousness does not actually do this, *i. e.*, in the example taken, translate the passage. The translation is not effected here. But later when my waking consciousness thinks of the problem again, these fragmentary thoughts of my secondary consciousness arise in my mind and with this information I complete the translation. The actual translation is put together by my waking consciousness. I am not conscious of the fact that these fragments of knowledge existed previously in my secondary consciousness. I do not remember a problem ever to have been solved by the secondary consciousness. It is always solved by the waking self, although the material for solving it may come from the secondary. When my waking consciousness solves it in this way the solution seems to come in a miraculous sort of way, sometimes as if it came to me from somewhere else than my own mind. I have sometimes thought, in consequence, that I had solved it in my sleep."

The subject of these observations was at the time in good mental and physical condition. Criticism may be made that the subject being one who had exhibited for a long time previously the phenomena of mental dissociation, she now, though for the time being recovered, tended to a greater dissociation and formation of subconscious states than does a normal person, and that the subconscious phenomena were therefore exaggerated. This is true. It is probable that the subconscious flora of ideas in this subject are richer than in the ordinary individual. These phenomena probably represent the extreme degree of dissociation compatible with normality. And yet, curiously enough, the evidence tended to show that the more robust the health of the individual the more stable her mind, the richer the field of these ideas. However this may be the very exaggerations increase the value of the evidence for the limitation of the extent, independence and activity of the subconscious states. If in such a subject we do not find, as is the case, evidences of subconscious automatism, excepting in absentmindedness, it is highly improbable that such activity exists in a perfectly healthy subject.

These observations are only suggestive, not conclusive. To solve the whole problem of concomitant, extra, or subconscious states, further and numerous observations are required, but conducted under conditions which shall exclude artifacts and abnormal states. It is interesting, however, here to notice that the direct evidence derived from these observations confirms the theoretical scheme of personal perception offered by Dr. Janet. That scheme is almost a literal representation of the facts as obtained by this method of experimentation.

Summarizing all the evidence which is at our disposal to-day, derived from actually observed facts, we may say, that *while a greater or less number of isolated dissociated states are constantly occurring under normal conditions, there is no satisfactory evidence that they normally become synthesized among themselves and exhibit automatism excepting in states of abstraction and as artifacts.*

A study of subconscious states is highly important for the determination of the mechanism of consciousness, and I am convinced that such studies will throw much light upon the problem of how we think.

At this time considering the fundamental importance of the problem of the subconscious, it has seemed to me wise to stop and review the evidence for the existence of normal dissociated mental states, and this for the further reason of the enormous part which these states play in pathological conditions and because of the credence which has been given to the theory of a normal subconscious self.

If the foregoing review is sound, it would seem that great caution is required in applying the inductions derived from a study of abnormal subconscious phenomena to normal conditions, and that the tendency has been to attribute too extensive a field and too great capabilities to this hidden mental life. The facts at our disposal do not support the hypothesis of a normal subconscious mind excepting within very strict limitations.

Nature of the Dissociating Process.

But the problem of the subconscious brings into stronger relief the still broader problem. What is the nature of the dis-

sociating process by which the duality is brought about? Is the explanation to be found in psychical or in physiological laws? It was a great advance to show, as has been done, that a large number of abnormal functional phenomena like anesthesia, amnesia, paralysis, aboulia, are all different types of the splitting of consciousness. They must, therefore, be due to some dissociating process. Janet interprets these different mental conditions as *chronic forms of absentmindedness*, a persistent failure of the personal consciousness to make more than a few syntheses. This failure is the consequence of exhaustion. The dissociation is, therefore, primary and the resulting automatism secondary. Janet is careful to point out that this is not an explanation. It is in fact only a classification. Breuer and Freud on the other hand would make the dissociation secondary to the development of what they call the hypnoid state, a group of fixed ideas, which are unable to make the synthesis with the personal consciousness.

None of these theories are satisfactory as explanations. Absentmindedness is not only insufficient as an explanation of the process, but even as a classification fails to take into account the differences in phenomena, such as the dissociation brought about as artificial abstraction by merely whispering in the subject's ear. I whisper in the ear of BIV and straight way she does not hear but inquires, 'Where have you gone to?' I speak aloud and she hears again. (The whispered voice is of course heard by a momentarily dissociated group of states which respond.) Why if this phenomenon is the same as absentmindedness, and is due to exhaustion, can not the 'personal perception' (Janet) synthesize the whispered voice as well as the conversational voice? Again multiple personalities with alternating memories are not exhausted, but can make any amount of other syntheses, including their own respective memories: Why not also with the lost memories of another personality? There is not a failure of *perception* of the ego, but a splitting of the ego itself. What has produced it?

Any theory to be sufficient must take into consideration all the facts not only of abnormal but of normal dissociations, including those artificially induced by experimental devices (sug-

gestion, automatic writing, etc.) When we do this we find in the first place, as already pointed out, facts indicating a normal process for dissociating consciousness, through which process normal and abnormal phenomena may be correlated. Normal absentmindedness, certain types of normal amnesia, sleep, spontaneous somnambulism, hypnosis, etc., can experimentally be shown to be types of dissociation, splitting of the ego, differing from one another in the extent and pattern of the fields of consciousness remaining to the personal ego. The process which brings these states about is probably fundamentally the same as that governing the abnormal splitting of consciousness.

In the second place a study of abnormal and induced dissociation shows that, while normal syntheses and automatisms largely follow psychological laws, the lines of *disaggregation* do not follow the lines mapped out by these laws. For instance they do not follow the boundaries of associated ideas.

The hand that performs automatic writing becomes anesthetic though the subconscious ideas which control the hand have nothing to do with tactile sensation. A subconscious fixed idea of fear of personal injury robs the personal consciousness of our subject M——l[1] of perceptions from the peripheral field of vision and from one half of his body. In another subject all the memories for a certain epoch in her life disappear in consequence of a shock. An emotional shock in A. P. excited by a slight fall during a high kicking act robs the personal consciousness of the power to move the arm and leg which are rigid in contracture. In Madam D. a subject of Charcot and Janet continuous amnesia for each succeeding moment of the day follows the announcement of a bad piece of news. There are no psychological associations in any of these examples between the ideas and the resulting dissociations, and psychologically we can find no reason why sensory and motor images are dissociated in one case, and memories in another. It would seem from the points of view of our present knowledge that we shall have to look for an explanation in some physiological process. All must admit that the final explanation must be in

[1] *Boston Medical and Surgical Journal*, Vol. Cl., No. 25, pp. 674–678, June 23, 1904.

terms of the neuron, in the dissociation of the as yet unknown neuron systems which are correlated with the psychological systems. But without attempting such an explanation, what I wish to point out is that the data of abnormal psychology go to show that the psychological disaggregation does not follow as much psychical as physiological lines. The cleavage is brought about by psychological influences — trauma — (ideas, emotions, etc.) but when the fracture occurs, it tends to follow the physiological map. Just as when a blow shatters a mineral, the lines of fracture follow the natural lines of crystallization, so, while a psychical trauma shatters a psycho-psychological system, the cleavage follows very closely the neuron association systems.

Thus when Louis Vivé passed into one state in which he had left hemiplegia, into another in which he had right hemiplegia, another with paraplegia, each with its own group of memories, the alterations can only be explained on the ground that these states were determined by some sort of physiological dissociating system. Likewise, when our subject M——l developed a complete amnesia for the English language, and understood and spoke only German, if we take into account all the phenomena, it would seem this amnesia was determined by physiological dissociation excited by a primarily conscious and later subconscious fear of injury.

Sally Beauchamp's general anesthesia is in no way the result of ideas psychologically in association with it. When BI and BIV exhibit a complete amnesia for each other's lives and exhibit their contradictory traits of character and physiological reactions, it must be because different neurons are brought into activity in each case.

BIV in hypnosis while slightly groggy from ether, talks intelligently and narrates the history of an adventure of the preceding day. I suggest to her that she shall open her eyes, wake up and be herself, a suggestion I have given a hundred time successfully. She opens her eyes and straightway she does not know me, or her surroundings, or who she herself is. An enormous dissociation of psychologically associated ideas has taken place, whether as the effect of the ether or some other

cause, I do not know, but according to psychological laws her syntheses should have been enlarged. I close her eyes again and she regains intelligence, remarking that — 'when my eyes are open I do not know who I am.'

On the other hand automatism and abnormal *syntheses* seem be be affected largely by psychical laws, particularly that of the law of association of ideas. Abnormal psychology then points strongly to the conclusion that there is a normal physiological dissociating mechanism which is the function of the nervous organization. It is this mechanism which brings about such spontaneous normal states as absentmindedness, sleep, normal induced states, like hypnosis; and through its perversions the dissociations underlying abnormal phenomena.

SKETCH OF THE HISTORY OF PSYCHOLOGY.[1]

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The science of psychology essentially reflects in its development the way the human mind has been able at various epochs to apprehend itself. The thought of any object is simply the conscious construction of that object; and this is as true of the sort of object — the mind — with which the science of psychology deals as of the object of any other science. As long, for example, as animistic views prevailed, a thoroughgoing positivistic treatment of the objective world was impossible; for the object constructed was not subject to regular law nor continuity of transformation and change. So also, as long as the animal body was considered an exception to the positivistic process, biology could not be a thoroughly developed natural science; for its object was a center of capricious and mystically motived changes. This is true of psychology, and more emphatically. For the object of the science of psychology is the mind, the object which it constructs from its own experience; that is, its object is just its own positive view of itself. We are accordingly led to see that the history of psychology is the history of the stages or modes of the evolution of reflective consciousness of self.

1. Greek Psychology.

The evolution of psychological views among the Greeks is capable of fruitful interpretation from this point of view. The earliest views were necessarily those possible at a period at which the dualism of mind and body — self and external world — had not been achieved. The so-called 'materialists' of Greece — who, just for the reason now given, would better be called 'protists,' 'pro-noïsts,' 'projectivists' (I shall use this last term), or something of like import — looked upon nature

[1] Read at the Congress of Arts and Science, St. Louis.

as the 'one,' 'the undefined,' a moving labile object (water, air, etc.). And it is characteristic of their views that they did not — because they could not — go on to make distinctions and differentiations in the lines of later more mature reflection. The period of their thinking in the history of opinion corresponds to the early a-dualistic or 'projective' period in the individual's personal development. The individual has a certain objective mass of material, 'protoplasmic' in a figure, in which the dual reference to subject and object is not yet attained. The world, to such an individual, is one of 'first-appearance' — not of matter and mind, nor of anything else which gives an antithesis of poles of reference. So the early thinking of the race was in this sense unreflective. The process of its theoretical interest did not lay apart its material in substantial categories; but it answered the question 'what?' by the assertion of the sort of predicates which were its possible objective constructions at that stage.[1]

The positive character of this first period, however, shows the transition motive to certain later dualisms: the character of animation, movement, change. In this respect, the Ionics suggest a further movement in the child's development. The immature reflection of the individual finds, in the perception of animation and capricious movement, the road toward a solidified and concreted dualism. Through this type of reflection the world-circle closes in somewhat upon the personal center. It neglects the fixed, changeless, inanimate things of the world, as in so far unexistent or hypothetical. In respect to them, the senses deceive. So in the thought of Heraclitus and Parmenides the becoming or change principle played its rôle, and the Greek mind began its career toward a form of dualism in which the 'fixed' was of logical or contrast value, mainly, not an objective category.

[1] This is not to say that the adult person himself — for example, such a thinker as Thales — was not self-conscious, and did not deal practically with the problem of self *vs.* things; but only that, in his reflection, he did not segregate the elements of his one general experience in explicite dualisms, nor consider the objects in the two spheres of practical experience as separate and distinct.

It may be explained here that I use the term 'object' (and its adjective form 'objective') of *any cognitive construction whatever* — anything that may be known or thought about.

In this general epoch, the 'projective,' in the development of Greek thinking, we must place also the νοῦς principle of Anaxagoras. It was a principle in the line of the vitalistic or change hypothesis; and it remained, indeed, only a postulate of order, movement, immanence in the world. It was not a subjective, nor yet an objective (a-subjective) principle. So far as it implied a dualism, it was one — that predominant one in Greek thought — of matter and form, not of subjective and objective. It may possibly be considered as in so far an unreflective anticipation of Aristotle's biological point of view — so much indeed is possible — but it was not in any sense an anticipation of the subjective point of view from which a science of psychology could isolate its peculiar matter. This accounts no doubt for its unfruitfulness in later thought.

The real isolation of the subjective or 'inner' seems to have begun with the Atomists, Leucippus and Democritus, in their famous doctrine of the relativity of the sense qualities.[1] This intuition led perforce, just as the same type of phenomena — the relativeness and deceptiveness of qualities, colors, odors, etc., in things — leads the child, to the wider question whether the 'inner' is not a sphere to be distinguished from the 'outer.' Indeed in Democritus this antithesis is actually and fruitfully made. His other great doctrine, that of the 'atoms,' was thus made possible, and has remained possible for all time; for by definition the 'outer' had to be stripped of those relative and ambiguous predicates which had embarrassed earlier speculation. The atoms could do their work in the body of external reality; and the mind could do its separate work of knowing that reality. This was a real advance upon the doctrine of 'elements' as held by Parmenides and Empedocles.

The subjective postulate thus once arrived at in the individualistic sphere of sensation, was to be carried out in the general sphere of truth by the Sophists; indeed it was forced upon them by the social and intellectual conditions which made men Sophists in their generation. In the Sophists began the play of certain forces akin to those which we find enormously germinal in the narrower sphere of the individual's personal growth. And in this our present method has further justification.

[1] Cf. Gomperz, *Greek Thinkers*, Vol. I., pp. 320 ff.

The growing consciousness of personal quasi-subjective detachment from the world of impersonal things, comes to the child through processes analyzed variously into motives of conflict, imitation, invention, discussion (and from the psychic point of view, introjection, absorption, realization) — a give-and-take or dialectical process between the individual and his fellows. In it all the essential fact of subjectivity in the actor's thought of himself and others, comes to birth. The actor becomes an agent; the observer, a creature of reflection; the spontaneous thinker, a possible amateur psychologist.

All this appears, there can be no doubt, in the Sophistic movement; and out of it indeed, the first race-psychologist was born — Socrates. In the views and methods of Socrates are focussed the rays which are to burn inward to the core of the human self. This appears true of Socrates in the following precise points.

1. The Sophistic principle *homo mensura omnium* (Protagoras), formulates the thought of an active and constructive center in the individual. The individual's or human nature's reaction to the world gives all the measure there is for things. In Socrates this principle was developed in an anti-individualistic or social sense.[1]

2. The contrast between 'opinion' (δόξα) and 'reason' (νοῦς), sharply brought out by the dialogue method in the hands of the master, Socrates, and developed by his disciples, now becomes more positive.

3. The view that truth is in general a thing of thought in-so-far eternal and immutable — not, as in the earlier transition stage, a function of a principle of change essentially indetermi-

[1] Against the individualistic interpretation for the Sophists generally, especially Protagoras, see Gomperz, *loc. cit.* I, 451 ff.

It is confirmatory of the parallel made in the text between the Sophist's and a stage in the individual's thought to note that Socrates' position was not in its nature individualistic, but was reached and maintained in the midst of social opposition and discussion. The Socratic method was a social dialectic or give and take. I do not know of any adequate exposition of the social — political, religious, etc. — factors which produced the Sophistic movement; but an account of a later analogous period — the rise of the Post-Aristotelian schools — is given in admirable terms by Caird in his *Development of Theology in the Great Philosophers*, II., Lect. XV.

nate in character. This is the germ of Plato's 'idea' in which reality becomes explicitly an ideal postulate.

4. In Socrates the way is opened to the form of dualism of mind and matter found in Plato's doctrine of matter (ὑλή).

Not stopping to develop these points — time does not allow — we may still say that Socrates was mainly a Sophist, not a clear subjectivist. He reached subjectivism only so far as it was involved in a dualism of the general (truth) and the particular (appearance), and that in an experimental and controversial way.[1] He did not realize the thought of mind as a psychic content in distinction from body.

Had Plato been possessed of the scientific interest this distinction might have been made then and there; for Plato deduced a principle of matter. But like Anaxagoras, with his postulate of mind, Plato's 'matter' remained a logical contrast principle, over against 'form' — a particular over against the general — not a concrete reality; and the philosophy of reality was to remain a rule of vibration between logical poles, rather than a synthesis of reflection.

So far as a science of psychology goes, Plato must be classed with Socrates in what we may call the period of 'experimental subjectivity.'

In Aristotle no less than in Plato, it is the outward movement of thought into reality that has the emphasis not the development of the subjective as psychic. This movement is that described in modern genetic psychology as 'ejection': the reading of the subjective into the external and the interpretation of the latter in terms of some aspect of the world of thought. This reached its clear statement in Plato's doctrine of 'ideas,' that is so far as the 'idea' itself was defined. It required a theory of the idea, however, only so far as that conception was to serve the metaphysical purpose. It did not require, nor did it receive, independent treatment, as itself object of scientific research or even as content of consciousness. The dualism, however, was only a mediating phase of the return to a deeper monism or

[1] The way which, when illustrated in the individual's development, is called the construction of a 'semblant' object — a matter of psychic experimentation with materials, akin to the child's playful and æsthetic imaginative constructions.

idealism: that of the unity of the particular and the universal. And in Aristotle, whose scientific impulse was strong, this reading of the subjective into the objective remained — in the doctrine of matter and form — a way of accounting for the organic character of the presented and objective world. It did not become a way of detaching the subjective. This is to say that Aristotle's point of view, in discussing the facts of mind, is more biological than psychic or psychological. Mind has definition as the form of the animal body; and while this implies a reciprocal definition of body — as material for the realization of form — nevertheless the emphasis is not on mind as such.[1]

Aristotle illustrates, indeed, an important fact in the history of science in general: the fact that positivism may be embodied in a scientific method before the criticism of the material is well advanced, and that the sciences of the objective order are usually well along before the corresponding sciences of the subjective order attain their emancipation. The reason of this limitation in the case of Aristotle appears when we turn again to the parallellism between the individual's and the race's growth in self-consciousness. The embodiment of the thought-content in things, by 'ejection' or, as the anthropologists say, by 'personification,' suffices for a theory of the world which is animistic and vitalized — for hylozoism, that is. But this does not go beyond Plato. The next step is to reach, with Aristotle, a naturalism of the objective order, by the correction and limitation of the animistic concept. This the individual does on his part by the return movement of his thought, whereby he re-absorbs a body of predicates into the 'inner' sphere. The psychic becomes, by this movement, the theatre of the more lawless, capricious, and unmanageable phases of appearance, and the world order remains what is left — the regular, the manageable, the lawful. The fixed, before neglected, now becomes the essence of things. It is no doubt a practical distinction at first, and only afterwards becomes the subject of that theoretical interest which develops its positivism first of all in the objective realm. So

[1] This is not to say, of course, that Aristotle did not make many valuable contributions to empirical psychology; he did. But still it is true that he did not develop a distinctly psychic method of treating consciousness.

the rise of science of the objective becomes possible. But not yet, evidently, can the psychic find corresponding treatment, as law-abiding and uniform in its movements; for if the inner sphere be constituted just by the segregation of materials inso-far practically unmanageable, the theoretical treatment of them also is baffled; and a science of these contents must await the rise of a reasoned positivism of the inner life.

It is necessary to point this out, for it explains certain negative aspects of later historical movements — and why psychology as a science of content was so late a growth. In two later world-epochs, in particular, and in their respective world-thinkers, something of the same situation presents itself. I refer to the rise of modern dualistic philosophy in Descartes, and the rise of Positivism of the stricter sort, in Auguste Comte.

I. The Dualistic Transition.

The transition to Descartes was made through the Stoics and the theologians of the Christian Church. The Stoics, reacting against the practical individualism of the Cynics and Cyrenaics, reached the concept of a sort of general self-hood which guaranteed law and order and virtue. This was a practical and eclectic rather than a reasoned attempt to overcome the dualism of their immediate predecessors.[1] The church theologians reasserted an individualism, but to them the individual became spiritual.

In these precursors of Descartes there was worked out a genetic motive which is unmistakable also in the individual's development: I mean the advance or progression from a dualism of 'inner-outer' to one of 'mind-body': from what may be called a distinction of attributes to a distinction of substances. The individual proceeds, in his generalization, to carry over the physical part of his own person — separating it substantially from the psychic part — to the side of the 'outer' as such. It is only when he is able to do this, *and does it*, that the dualism of mind and body is anything like complete. The substantializing of the mental principle which has so far proceeded by certain curious stages — being variously a refined physical

[1] Cf. Caird, *loc. cit.*, Lect. XVII.

something, a breath, the limiting notion and form of matter — now finally becomes the hypostatized substance which bears the psychic qualities. The substance soul does finally become logically detached, but mainly for theoretical and doctrinal purposes;[1] for even then soul and body remain in so far attributal to each other, that either can be predicated on occasion as either cause or effect with reference to the other. This was notably true in the entire church development; and the view is still dominant in theology. This cause and effect bond is the last one that remained to be loosed.[2]

In Descartes, for the first time in the history of thought, certainly of occidental thought, is the psycho-physical problem specifically set in the form of the conception of a natural relation between mind and body, considered as two separate substantial principles. The problem becomes: what is the relation? It assumes not only the dualism of the two terms, but their actual separation. Descartes not only reaches such a dualism, but he sets up the full relational problem of mind and body. And further, he identifies the spiritual principle with 'inner experience' or 'thought.' He is in advance of the church philosophy in this important respect, that while, to the latter, it was a problem of *separating mind and body*, to Descartes it was a question of *bringing them together again*. Descartes said that interaction was impossible; and the theory of preëstablished harmony was the alternative.

Why then, it may be asked, did not a purely naturalistic psychology begin with Descartes? For much the same reason, I surmise, that it did not begin with Aristotle: because Descartes did not conceive the inner principle, the soul or thought,

[1] The earlier crass doctrine of transmigration as in the Orphics and in Empedocles did not involve a reflective dualism; for the soul was not defined as a principle. When the dualism arose, however, such views availed themselves of so much support, just as modern theology supplies a doctrine of immortality in support of the early anthropological belief in a world beyond. Put in psychological terms, we may say that such early religious and anthropological views were object of practical and, in some cases, æsthetic interest, but not of the sort of theoretical interest which leads to philosophical enquiry.

[2] It is pointed out elsewhere, PSYCHOLOGICAL REVIEW, May, 1903, that the case of mind and body is the last instance of the sort of commingling of substances and forces involved in interaction theories.

in terms of continuous and lawful change. Just in this was it contrasted with body. Extension is the sphere of geometry and physics; thought is the source of spiritual manifestations; and these two domains of fact, though parallel, are essentially heterogeneous. That this is true of Descartes is proved historically; just as the corresponding fact comes out in the comparison of Aristotle with Socrates. In each case a monistic idealism followed, not a scientific naturalism. Socrates was followed by Plato, Aristotle by a new mysticism, while Descartes led right on to Spinoza. In each case, we find an attempt to transcend the specific form of dualism of its own period.[1]

III. The Postulates of Modern Scientific Psychology.

From the preceding exposition, I may venture to draw certain inferences of a negative sort: statements of what the thought of the earlier centuries lacked; and follow that with the positive characters belonging to the nineteenth century science.

What the earlier thinkers lacked, then, was (1) a full naturalism in their point of view: a naturalism which could follow only upon a critical dualism of mind and body. Grant the dualism of inner and outer, take the further step to that of mind and body, then — and this is the needful thing for naturalism — admit the oneness of the knowledge of nature as a whole in the face of the cleft in nature which the dualism postulates. The thinkers we have been considering did not achieve this last step. They worked out their theoretical interest by establishing a philosophical solution of the dualism, or on the other hand, by an æsthetic handling of it.

(2) They did not achieve a positive way of treating all data as material of knowledge as such, material to be progressively systematized and enlarged by research. The former is the full scientific point of view; the latter is its method and instrument.

[1] It is an interesting point that in each such case, the supposed reconciliation is not logical but, in a broad sense, æsthetic: the motive in Plato is poetic, in the Post-Aristotelians it is mystic, in Spinoza it is religious — a matter it would be well to expound in its own place. It has its parallel, moreover, in the individual's mode of treating his dualisms, *i. e.*, by the construction of objects which are valid from æsthetic points of view. This is, I think, the normal genetic outcome.

What modern psychology has in addition is just the something that these early thinkers lack:

1. *Naturalism:*[1] or the view that all events or phenomena whatever are part of a natural order, and are subject to general and ascertainable rules of sequence.

2. *Positivism:*[1] or the view that a methodology — a theory and practice of method — of research is possible, for the discovery of the rules or laws which govern the sequences of the natural world.

Both of these scientific postulates hold for psychology. They have long been established in the physical or exact-quantitative sciences; they have been slow of formulation in the biological sciences; they are only beginning to have adequate recognition — notably, the second of them — in the mental and moral sciences. It is the characteristic feature of nineteenth century psychology, that it has developed the first of these postulates fully and the second partially.

IV. History of Nineteenth Century Psychology.

The nineteenth century opened at a natural pause in the evolution of theories about the mind. In the flow of the great currents, certain eddies had formed late in the eighteenth century. The dogmatic movement in Germany had passed over into the critical; and Kant had attempted a new æsthetic reconciliation of the dualism of inner and outer. The Kantian psychology or anthropology is essentially a renewed subjectivism — that is, so far as it is 'critical.' Neither scientific naturalism, nor positivism in the sense defined above, profited greatly from the work of Kant. Indeed the explicit attempt to refute Hume throws the weight of Kant as authority — to go no deeper — on the side of an essentially obscurantist attitude toward facts. Note the arguments in favor of *a priori* space and time, which very little careful observation would have materially modified. And historically Kant led the way to what Höffding calls the 'romantic movement,' from Fichte to Hegel.

Again, in France an impulse was asserting itself away from

[1] It should be noted that I speak of *scientific* not of *philosophical* naturalism and positivism.

the materialism of the sensationalists toward the naturalism of Rousseau. Rousseau's recognition of the psychic involved a truer naturalism than the view which denied the life of ideas and of all higher functions in favor of a sense-process materialistically interpreted. Neither Rousseau nor Condillac, however, combined both the two postulates.

In England a science of psychology was emerging at the opening of the nineteenth century. Locke had broached a subjective naturalism, which the French sensationalists, as I have just intimated, developed on one side only. Hobbes was a positivist in much the same sense for our purposes as Comte. But in David Hume the two requirements of a true science of psychology were consciously present. Hume treats mind as a part of nature — this is naturalism — and he also works at the problem of discovering the laws of mental change by actual observation — this is positivism. He is justified in both by his results; he is further justified by his extraordinary historical influence.

If then we are justified in saying that David Hume is one parent of the science of psychology — in the sense of the word that places this subject in line with the other natural sciences both as to its material and as to its method — then we have to look for the other parent, I think, to France. Dropping the figure, we may say that in Rousseau, France contributed an essential moment to the development of the science. Possibly this contribution should be called the Rousseau-Comte factor; as possibly also the British contribution should be called the Locke-Hume factor.

The influence of the Rousseau-Comte factor, which is to-day more undeveloped than the other, but is now becoming fertile, may be shown by an appeal again to the analogy with the individual's growth in personal self-consciousness. And as intimation of my meaning, I may refer to the Rousseau-Comte *motif* as the 'social' or 'collectivist,' and to the Locke-Hume *motif* as the 'personal' or 'individualistic.'

Taking up the genetic parallel, we may remark that the development of the positivistic postulate by Locke, Hume, and the Mills, *in an individualistic sense*, has proved inadequate,

so far as it claims to exhaust the psychic matter. In the development of the individual the rise of the thought of a separate personal self is a late outcome of reflection. The early stages of dualistic thought are in so far social that the mind-body dualism is an abstraction in both its terms. Mind is many minds; and body is many bodies. The material of self is collective and distributive, not unitary nor individual. The child thinks self as a term in a social situation.

If this be true, the science of mind must be one in which the abstraction of an isolated individual mental life should be used as an instrument of method rather than as a truth of analysis and explanation. And there should be a science of psychology in which the material is, so to speak, social rather than individual. This point has been worked out only in recent literature, but its advocates may find the source of this type of view in the French thinkers now under discussion.

Besides these two great movements, credited respectively to Great Britain and France, modern naturalistic psychology has had two important impulses. The first of these came about the middle of the century in the rise of the evolution theory, and from the side of biological science; the other from German beginnings, and from the side of physical science. I shall speak of these respectively as genetic psychology, finding its pioneers, Lamarck and Darwin, in France and England, and experimental psychology, founded by the Germans, Fechner and Lotze.

The various factors now distinguished may be taken up briefly in turn for consideration. I shall treat them under the two larger headings already set forth: *Naturalism*, comprising (1) the British movement called above the Locke-Hume factor (empirical psychology), and (2) the French-British evolution movement (genetic psychology); and *Positivism*, comprising (1) the Rousseau-Comte movement (social psychology) and (2) the German experimental movement (experimental psychology).[1]

[1] These two headings are indeed not exhaustive nor mutually exclusive. The viewpoint respecting the material cannot fail to influence the method; nor the method the selection of material. For example, the Rousseau-Comte current is a direct gain to naturalism no less than to positivism; and the opposite is true of the Locke-Hume movement.

Before proceeding, however, it may be well to give a résumé of principles — the platform upon which the entire development is projected. This platform is that of cognitive and reflective self-consciousness of such a sort as that which the individual has attained when he thinks of his inner life as a more or less consistent unity, passing through a continuous and developing experience: a self different from things, and also different from other selves; yet finding its experience and exercising its functions in closest touch with both. And furthermore this touch with things and persons is so close that, whatever his reflection about himself may lead to, he accepts the facts, (1) that the world as a whole *includes himself and others in its larger uniform processes*, and (2) that the methods of its treatment of him *through his body*, are also his methods of handling it. The individual must be, that is, *first*, a somewhat careful naturalist, and also *second*, a somewhat skilful positivist; and it is only when there is the reflection of *this sort of self-consciousness into the scientific endeavor of the race* that there comes a time ripe for a truly scientific psychology.

IV. Ninteenth Century Naturalism.

British Empirical Psychology. — The empirical movement reasserted in John Locke the subjective point of view reached in the dualism of Descartes. Furthermore, it attained in David Hume the return movement from a pure naturalism of the objective only to a corresponding naturalism of the subjective. Locke's subjectivism is seen in his doctrine of primary and secondary qualities, in which he renewed the relativity of Democritus and the Cynics, and in his polemic against innate

The scientific treatment of mental diseases is also a most important matter, which should be classed under positivism or positive method. It is not within my province — nor is the time ripe, I think — to estimate it. Its development is one of the great tasks of the twentieth century (cf. Meyer, *Psychological Bulletin*, May–June, 1904, for an exposition of present-day tendencies and theories).

As it happens, it fell to the present writer to draw up a report on psychology for the other great American exposition, that at Chicago in 1893. That report, entitled *Psychology Past and Present* (published in the Psychological Review, and now incorporated in the volume *Fragments in Philosophy and Science*) goes into greater detail respecting recent movements and literature, with special reference to conditions in the United States.

ideas. Hume's subjective naturalism appears in his entire work. Hume's theories of ideas, belief, substance, cause, all testify to his complete absorption in the thought of the psychic as a law-abiding and continuous flow of events.

The most explicit result of this point of view appeared, however, in the theory of Association of Ideas, upon which the school of British empiricists founded their psychology. James Mill, J. S. Mill, Thomas Brown, and Alexander Bain are the figures which are drawn large upon the canvas of associationism in the nineteenth century. The theory of association, considered as a formula of general explaining value, was epoch-making historically, inasmuch as it was the first general formulation made from the new point of view.

In France, something in some degree analogous appears in the writings of Condillac and his associates before the voluntaristic reaction of Maine de Biran and Jouffroy. The postulate of sensation was indeed a naturalism, as has been said above; but it was not motived in strict philosophical neutrality, nor did it issue in a general formula. At the same time it served to establish the Lockian tradition on the continent, and to furnish a shibboleth which, though destructive enough from other points of view, nevertheless helped to clear the way to a saner empiricism. It should be noted, too, that there were in Germany sporadic intimations, and more, toward a fruitful naturalism; but that these remained without great influence — notably the remarkable work of Beneke — and had to be reformulated in later times, shows that, as matter of fact the naturalistic movement did not receive any indispensable support from Germany.[1] Beneke's advanced positions, it is fair to add, are only now becoming generally known as anticipations of certain important genetic principles.

The outcome of this great British movement is an established empirical tradition. The gain is seen, on one side, in the soil tilled for the sowing of evolution seed; it appears again in the established spirit of patient research which is the life-blood

[1] Indeed, this might be put more strongly; for the era of the Enlightenment in Germany brought a reaction toward the more mystical evaluations of experience based on feeling — *e. g.*, in Tetens and Schleiermacher.

of science. In Alexander Bain we have the summing up of the results for the whole mental life; as in Herbart, in Germany, we find them illustrated in a new Intellectualism, and in Herbert Spencer, their further development on a Lamarckian platform. In Spencer, it is true, the psychological point of view served the need of a larger philosophical purpose; but he shows that the naturalistic habit of mind had become so fixed that the association psychology could be recast on evolution lines, while claiming still that violence had not been done to its essentially empirical spirit. A later author, in whom the positivistic method is well realized, but in whom the genetic spirit is not fully developed is William James; and still another who will be named below as one of the pioneers of the experimental psychology, Wilhelm Wundt, is not only not genetic in his naturalism (being neo-vitalistic), but has also a corresponding limitation upon his method, in spite of its positivistic claim (being somewhat obscurantist in his demand that psychology shall yield support to a philosophical voluntarism).

French-British Evolutionism: Genetic Psychology. — The rise of the evolution theory in biology supplied the direct motive to a genetic psychology. Lamarck himself recognized the psychological factor in one of his general principles — that in which he formulated the function of mind as effort, struggle, etc., in modifying the organism to accommodate it to the environment. The explicit application, however, of the Lamarckian theory was due to Herbert Spencer in whose work we recognize a conscious attempt to work out an evolution theory of mind, as a branch of general cosmology. It is interesting that it was in the same generation, indeed in the same decade, that those other Englishmen, Darwin and Wallace, gave both biology and psychology alike an impulse which has established a genetic science. For Lamarckism is not positivism; only in Darwinism did a thorough-going positivism of method supplement and correct the naturalism of Spencer and Lamarck. The contribution consisted in the extending to mind of the methods of positive and comparative research, and the formulation of a principle, that of natural selection, which established genetic continuity and by which research has since been directed and

controlled. It is somewhat remarkable that Lamarckism never secured the hold upon the minds of psychologists that it did upon those of biologists; and the progress to Darwinian positivism has had real reinforcement from workers in our science.

Now — at the end of the nineteenth century — the genetic principle is coming into its rights. It has done most service hitherto negatively, in its antagonism to a psychology exclusively associational, on the one hand, and to one exclusively structural, on the other hand. The earlier science was debtor, in its structural concept, to physics; it was a positivism of the atomistic or a-genetic type. The latter is debtor, in its functional concept, to biological science; it is a positivism of the developmental or genetic type. However fruitful the atomistic, structural psychology has been, it has had its word, and it is not the final word. A great era of research is upon us in the treatment of consciousness as a thing of functional evolution in the race, and of personal development in the individual. The general psychology of the future has been prepared for in the physical mode of psychologizing, just as the general biology of the present was prepared for by the anatomical science of life which preceded it.

Among those whose names should be mentioned as contributing either to the Lamarckian or to the Darwinian form of the genetic principle are Häckel and Weismann in Germany; and among those powerfully aiding its acceptance in their respective countries, are Ribot in France, Morselli in Italy, Romanes and Huxley in England, and John Fiske in America.

V. Nineteenth Century Positivism.

French Positivism; Social Psychology.—In France the progress of naturalism, in matters psychological, was much more rapid, and its victory more complete, than in England and Germany. This difference is due I think to the different attitudes taken in these countries respectively toward the theory and practise of religion. In France, the theological bias and restraint, in which a certain conception of the mental principle was involved, were done away with before and during the revolution; and a positive scientific method was resorted to, to

replace the theological — as witness Comte's actual attempt at a religion of humanity. In England, Germany, and America, on the contrary, while the growth of naturalism has gone on apace, the actual realization of scientific method has been slow and difficult. Such a step involves the giving up of vitalism and the theory of interaction of mind and body, together with other formulations in which the theological spirit has lately taken its stand.

In Auguste Comte we have a thinker whose dualism was ripe for a scientific psychology, but who nevertheless failed to achieve the point of view of law-abiding subjective change. Comte was, indeed, *assez positif* in his claim. He took up the problem of an independent science of psychic processes; but from failure to recognize the subjective as such, denied its possibility. His objective monism is seen in his view that it is through the objective or positive series of facts, biological and social, that the psychic series is to be done justice to—classified, arranged and explained.[1] It is the reverse swing of the pendulum to that of subjectivism, though from a different theoretical support. It does not solve the dualism; as the idealistic monisms of Plato and Spinoza did not. And it parallels practically the same stage of individual reflection as these systems: that which recognizes the futility of the half-mature dualisms of practice and common-sense. But in Comte the practical and the methodological were prominent, and he was urged on to justify the sort of naturalism in which these two motives issued. This he did by asserting the essential fragmentariness and capriciousness of the psychic as such; while he should have held to a larger naturalism, in which the external and the psychic each develops its own positive method.[2] Of course it is no reconciliation of two terms to deny one of them; and such a procedure has not the merit of æsthetic synthesis which we find in the great monisms. But nevertheless, the assertion of the universal claim of positive method was of the first importance: it

[1] His inconsistency is seen in his appeal to the subjectivism of Kant's relativism of knowledge, to refute metaphysics, while using the objective order to refute the subjective point of view of Condillac and the spiritualistic school.

[2] This was done by the school of English positivists who followed Comte in his attitude toward metaphysics.

carried forward one of the great naturalistic movements of history.

While the fruitfulness of the positivism of Comte accrued to science in general, not directly to psychology, yet it was only his personal convictions that hindered his coming into the psychological heritage as well. As it was, the spirit of his teaching awaited its working out in a later generation. It was to the profit of sociology; for his negative answer to the question of positive psychology was possible only because of his affirmative answer to that of social science. The positive bearing of Comtean positivism comes out therefore in two ways: first, as announcing a general method, and second, as preparing the way for a social psychology which should reconstitute part of the domain assigned to sociology — that of psychic and social experience — in a separate science.

As to this latter undertaking — the isolation of the content of social psychology — the requirement had already been met, in spirit at least, by Jean Jacques Rousseau. In Rousseau, to whom French naturalism owes its main impulse, we find two contrasted and in a sense opposing points of view, one positive and the other negative. These together tended to the segregation of a certain sort of material. These positions were, first, the positive 'return to nature,' which took the form of individualism in politics and education (in *Social Contract*, and *Émile*), and, second, the theory of the 'general will,' which opened the way for a new collectivism, whenever its implications for social psychology should be brought out.

These positions of his predecessor might have led Comte into a truer view, and have brought about the establishing of a social psychology — a science of the 'general will' — in the spirit of the motto 'back to nature.' But this, as we have seen, Comte did not realize.

Undoubtedly, however, there is a profounder reason for the immediate unfruitfulness of the work of Comte — and this is my justification for dwelling so long upon it. Pursuing the method employed above, we may still recognize the requirement that the science of mind follow the genetic stages of the individual's growth in self-consciousness. With this cue, we may say that

it was impossible that a psychology of social collectivism could be established before a theory of psychic individualism had been fully worked out. The individual is, indeed, truly a social person from the start; but this he himself does not recognize until he has lived through a period of strenuous unreflective self-assertion. Moreover, even then this consciousness of his social place is not in itself the adequate impulse to the theoretical interest to explain it. So social psychology which embodies just such an interest must perforce await the development of individual psychology and then serve to supplement it. We are able to see this now, inasmuch as we are only now realizing the transition from the latter to the former; and it is for this reason, also, that we are able to see why it was that both in France and in England the repeated claim of collectivism, both social and political, was negatived and outlawed. Hobbes must yield to Locke, Comte to Mill and Spencer; and only after these latter, could Bagehot and Stephen and Tarde arise, if indeed the renewed collectivism was to have a psychological foundation worthy of the name. And it is equally true that it is only as we work out the genetic processes whereby the reflective social self of the individual justifies its right to succeed the individualistic, that we can hope to see how society can rationally hope to reconstitute itself as more than a group of competing individuals. For having begun this work later psychology, notably in France and America, deserves praise. But it can succeed only as it maintains both the naturalistic spirit and the positivistic method of Comte.

German Positivism: Experimental Psychology. — The establishing of laboratory psychology is usually and rightly accredited to the Germans; but it is not so usually seen that this work does not involve a new point of view. On the contrary, it is the culmination of the positivistic movement sketched above. It not only admits the place of mind as a part of nature, but it suggests the employment of the methods of physical and physiological science. It arose in Fechner's attempt to discover the law of connection between psychic and bodily events. Such a law once made out, research would be guided and also controlled by its recognition.

Apart from the fact that the attempt failed, so far as Fechner's investigation was concerned, the importance of the conception cannot be questioned. A later formula — that of psychophysical parallelism — is indeed truer to the ideal of a working positivism, just from its negative and colorless character. But ignoring points of controversy, we may still say that many fruitful researches have been carried out in this field; and disabusing ourselves of too great optimism, we may still count laboratory work as a part of the heritage bequeathed to the twentieth century. No doubt we are to see fruitful formulations under the rule of which great discoveries are yet to be made. Together with the actual founder, Fechner, we should name Lotze as also a pioneer in experimental psychology, and Wundt as an effective builder upon their foundations. Other great names in this connection are those of Weber and Helmholtz.

VI. Prospects.

In conclusion, it may be deemed proper to set forth the probable lines of development of psychological enquiry in the opening century.

In the first place, it is clear that both naturalism and positivism — spirit and method — are to survive in psychology, as in science generally. And for the reading of their future development we may again appeal to the rule of individual development. Certain lines of probable advance may thus be discerned.

1. The thought of the unity of social content is a great step toward the breaking down of any associational or other 'privately conducted' science. The psychology of the future will be social to the core; and its results, we surmise, will be revolutionary in logic, sociology, ethics, æsthetics, and religion — the disciplines which are built upon psychology.

2. It follows that the position that the private psychic point of view is the only valid one is to grow more and more obsolete among workers in this field. It will no longer be possible to claim that all truth about mind must be traced in some individual's consciousness, and that the laws of the science are to be those of observable psychic continuity alone. Psychic events are intertwined with physical and biological events, and their

sequences involve objective as well as subjective terms. The two sciences which will for this reason be brought into vital relation with psychology are physiology and sociology.

The two lines of development just mentioned are guaranteed by the essentially social — and by corollary, un-private — character of our higher reflective processes.

3. The genetic point of view will be worked out in a method of research by which genetic science will take its place beside quantitative science: psychology will become largely genetic or functional. The method in the biological sciences brought in by the theory of evolution consists essentially in the tracing out of genetic sequences; a thing is defined in terms of what it does and becomes and of what it arose from. The anatomy of structure is only a restricted and largely descriptive branch of general biology. So psychic processes are to be understood as phases of a continuous function; their meaning is in what they do or become and in what they arise from. The analysis of a cross section of consciousness is either descriptive and thus barren of further results, or it is hypothetical and in so far possibly mythological. This is the essential defect and the dilemma of a 'structural' psychology.[1]

The genetic movement is guaranteed by the current demand and need that the dualisms of partial reflection embodied in the older science be overcome. Only as a law of genetic development is realized can the postulates of self-consciousness at this period or that be justified. But the justification of one such set of postulates is, in each case, the abrogation of a former set, and the prophesy of a later set. The law of the whole series as such it is the task of genetic science to establish. It is no longer possible to rest content with a science of body in one text-book and a science of mind in another text-book, each of which claims that no single text-book can be written from a point of view which explains the origin of the dualism of the two, and sets forth the goal at which the dualism is finally explained.

[1] It may be observed that even the association psychology was preferable to the modern attempts to reach a psychic atomism, and from these to construct the mental life; for the law of association deals with concrete actual units, and formulates real psychic happenings.

Apart from private speculation, it is psychology alone which can solve this problem; since it is psychology alone in which the very movement itself by which the sciences are differentiated writes itself down as a form of reflection. The origin, the motives, the object, the goal of thought itself are just the content of psychology; psychology must become, therefore, more and more the interpretation of and reinterpretation of the genetic movement of the entire thought content.

4. Involved in the two lines of progress just indicated — the social and the genetic — and also confirming our expectation regarding them, there will be a racial and comparative psychology. In racial evolution the human genetic series is objectively worked out; and in the animal world, treated by comparative psychology, the corresponding pre-human series is displayed. Here psychology will come into vital contact with ethnology, on the one hand, and with animal biology, on the other hand.

Thus described, the work of the nineteenth century in psychology has been indeed most important. It has established the science; it has set the direction of its future movement. It remains for the twentieth century to reach practical applications of its results, and to improve the methods and instruments of further discovery. The present outlook is that social psychology will be carried on in France and America, genetic psychology in England and America, experimental psychology in Germany and America.[1] And such an expression is only what may be put more explicitly in the form of the opinion that in no country is the outlook so bright for the science in all its branches as in the land of the Louisiana Purchase Exposition of which this Congress of Arts and Sciences is the most interesting and perhaps the most remarkable part.

[1] In Italy the principal currents set toward pathological and physiological psychology — tendencies which are also strong in France.

AN ANALYSIS OF ELEMENTARY PSYCHIC PROCESS.[1]

CONTRIBUTED FROM THE PSYCHOLOGICAL LABORATORY OF OHIO STATE UNIVERSITY.

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The primacy of cognition among the so-called mental faculties is a tradition of long standing in that group of studies to which the term psychology is generally, but loosely, applied. The reason for this may be found in the fact that the chief interest that has determined the development of these studies was philosophical rather than scientific, and the validity of knowledge, not the process of knowledge, came to be the vital point on which other groups and branches of these studies centered. Not to go farther back than the founder of modern empirical psychology, we find that Locke, while he introduced us anew to a face-to-face acquaintance with the facts of our own minds, was dominated as much by the certainty and extent of knowledge as he was by the contents and relations of the knowing consciousness in the *Essay Concerning Human Understanding*. The probable origin of the Essay in a series of discussions relating to the moral life suggests the complex motives of the author, and the subordination of the analytic and descriptive features of his work. This view of psychology which makes it a subdivision, or a special method, of an inter-related series of reflective studies has more than historical significance. It is variously held by writers to-day who perhaps would agree among themselves in no other single point. We have no quarrel with those who consciously work out their psychology in dependence upon some broader and more human interest, such as epistemology or

[1] The writer wishes to acknowledge the assistance of his colleague, Dr. T. H. Haines, in collecting part of the data on which this paper is based. For the use to which it is put the writer alone is responsible.

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ethics, and for these we can understand why cognition should not only bulk larger, but be primary in their psychological theories. In another way than this has tradition vindicated itself; for among those who claim for psychology its right to an independent study, and have cultivated it according to the most modern scientific methods, cognition, by some, is still accorded its historical position, and an intellectualistic cast is given to their treatment of mental process throughout. The most conspicuous example is Professor James, who claiming for sensations a distinctively cognitive function, affirms, in his definition, that they are '*first* things in the way of consciousness.'[1] This may be, or it may not. It is a question of fact which cannot be decided by an appeal to history, and one on which an opinion, pious or otherwise, has no particular bearing. The importance of the present study consists in definitely raising the question of fact, for it seeks an answer to the inquiry, What *are* the first things in the way of consciousness? It is, in other words, an attempt, experimentally, to determine what, according to the general view of the subject in relation to which we are bringing our own special problem, is the nature of the *primum cognitum*, if and so long as *cognitum* be not too narrowly interpreted, as, for example, to exclude the possibility of the *primum* being something else than cognition. To put it in another way, our question is, What is the character of our most elementary conscious experience?

The feeling out of which such a question comes, we admit, is not new. As we have said, Locke was side-tracked through other than purely scientific interests. But it is interesting to note that something very near to the present inquiry is implied

[1] It should be needless to say that we do not necessarily imply that Professor James with his abhorence of the 'Kantian machine shop,' is to be included among those who have given us an exclusively intellectualistic psychology. But if the 'first' forms of consciousness are cognitive in character and there is to be genetic development throughout, we do not see how you are going to avoid subordinating the other functions by making them dependent on this one. You may, of course, introduce them as independent factors; but psychological pluralism does not answer the scientific question involved in the demand for explanation, if it is as far as you can go in the way of description. As I have said in the text the first question even for descriptive psychology is, Are the first forms of consciousness of a cognitive order?

in the theory of Leibniz concerning the grades of knowledge to which he was led through his doctrine of monads. Remaining under the intellectualistic tradition of his predecessors, there is yet an attempt at a closer analysis of the field of cognition, so as to get at the elements of consciousness, in his postulation of *petites perceptiones* to account for those forms of consciousness which do not exist as clear and distinct ideas. The importance of the distinction thus drawn is best seen in the work of Baumgarten who took the suggestion of Leibniz and founded on it a new science. Recognizing with his contemporaries that logic is a theory of the higher forms of knowledge, Baumgarten contended that gnoseology, or the science of knowledge, was incomplete until it had provided a theory of the lower forms as well. This theory he sought to formulate in what he termed æsthetics. Æsthetics, therefore, is primarily a branch of — as we should say — epistemology, and it is distinguished from logic, which has to do with clearly perceived truth, because, while it still is an investigation of truth, it is confined to truth under particular forms — to truth confusedly, not explicitly and clearly, apprehended. Such a distinctive class of truth, Baumgarten holds, we have in all our judgments of beauty. Æsthetics, consequently, is not only a department of knowledge, but also a theory of the beautiful. This connection of interests that seem to be wholly analytical with the formulation of those questions that center in the artistic side of life is mentioned here because the writer has come, in part, to the problem of this paper through a study of simple æsthetic reactions the results of which were published, in collaboration with his colleague Dr. Haines, in the PSYCHOLOGICAL REVIEW.[1] Here it was shown that judgments of appreciation are in themselves much more elementary and immediate than those judgments of beauty which unmistakably involve a cognitive element. It would seem to follow, therefore, there are forms of conscious reaction which are more primary than cognitive reactions, and, independently of the latter, are sufficient for the guidance of conduct. These conclusions certainly put the

[1] T. H. Haines and A. E. Davies, 'The Psychology of Æsthetic Reaction to Rectangular Forms,' PSYCHOLOGICAL REVIEW, July–September, 1904.

present inquiry in a new and interesting light. In this connection, our task is simpty to work out, as far as possible, certain of the implications of the former study in an experimental way. The results of this paper, consequently, will have an important bearing upon the psychology of the æsthetic consciousness in general if it in any way makes clear what is the character of the more immediate and primary processes of mental life.[1]

The great advances that have been made during the last quarter of a century in the formulation and study of biological questions has not been without its influence upon both psychology and philosophy. As concerns psychology there has been, among the biologists, a tendency to annex the field of consciousness, and among some psychologists the grand ambition to lay hands on all vital phenomena as a legitimate field in which to exercise their psychological enterprise. These are extremes which but indicate the close relationship that many have come to feel exists between the two sciences. Among those who take a saner view of the mutual helpfulness of the work done in both these fields Principal Lloyd Morgan, who is primarily a biologist, and Professor Mark Baldwin, who is primarily a psychologist, are entitled to first recognition. Each in his own way has led us to investigating the kind of problem that is here undertaken from the experimental side. But so far as we know biological interpretations of mental phenomena have remained within the realm of theory, as for example in the articles of Professor H. H. Bawden, and have never been submitted directly to the test of facts brought to light by the laboratory method. The writer has to confess that it was the desire to scrutinize more carefully a certain view of the nature and place of attention in conscious experience to which

[1] It may be well to note here that the conclusions of an experimental study of this kind will be of assistance in deciding between the two views of æsthetics which engage the largest share of attention at the present day. These are the intellectualistic and the emotional views. But, of course, beforehand and independently of some such inquiry as the present one, we can decide, not on a basis of fact, but merely on the basis of temperament. This is a similar limitation to that placed upon ourselves in connection with the supposed primacy of cognition among the mental faculties. Here as there, no extrinsic interest must be allowed to influence our proceeding. It is, before all, a matter of fact with which we are concerned.

he had been coming, and which seemed to owe something to the influence of his studies in the field of biology, that led, in part, to the particular problem with which this paper deals. He was anxious to know whether his conclusions already tentatively reached were experimentally verifiable. If they were, the independent line of evidence thus brought forward would tend to confirm his position; if they were not, his theory would have to be held with less confidence, certainly modified, and perhaps abandoned. But this bit of personal detail is important not merely as helping to define the problem more specifically, but also as going to show that the newer phases of psychological study may be mutually supporting and corrective. They are, however, the experimental facts which are our chief concern whether they are to be given a biological or quasi-philosophical explanation. Primarily, we are concerned to know what is the character of our most elementary psychic experience.

II.

All the experiments were conducted in the dark room. The eyes were the only organs of reaction. The observer was seated in a chair before a black cardboard screen placed in a vertical position 1.3 meters away from the eye of the observer. The source of illumination was a sixteen-candle-power light enclosed in a box directly behind and above the observer's head. The light was emitted through a three-quarter-inch hole covered with tissue paper. This light was controlled by means of a switch by the experimenter who sat behind the screen. He could give a longer or shorter exposure as circumstances directed or his purpose suggested, but the aim throughout was to limit the experiments to momentary stimulations. Besides, no two successive experiments were made so close together in time that perfect accommodation to the dark had not been restored. In this way the interference of after-images was successfully avoided.

The figures to which the observer was required to react were made of white card board. They were not, with a single exception, larger than 100 mm. in the greatest dimensions. The purpose of this was to avoid overtaxing the attention by making

too great demands. Under the same conditions, the same figure twice the size would be likely to confuse and so dam up the mental process rather than elicit it in normal fashion. Perhaps more than twice the exposure limit would be needed in such a case, and this would give time for a larger number of irrelevant impressions to gain entrance to consciousness giving rise to numerous complications which it would be difficult, if not impossible, to properly allow for in the results. A series of different figures were used, so that no observer knew beforehand what he might expect at any given time. Moreover, he was not allowed to see the figures before they were displayed on the screen. The limit of his knowledge was that some form would be exhibited, and he was asked to describe as accurately as he could what consequently took place in consciousness. Meanwhile he was engaged in conversation to eliminate as far as possible the influence of a heightened expectation. Pre-adjustment in this way was reduced to its simplest condition.

At one time it was thought desirable to use auditory, as well as visual, stimuli, introducing for this purpose a number of instruments of different timbre. This would have obviated any objection from pre-adjusted consciousness to a given form of stimulus. But what would have been gained in this point would have been lost in others. It would, for example, have put the oberver under a strain in the preliminary stages of the experiment and so confused, through complicating, the problem for him. Besides, it would have put the two stimuli on a different footing by requiring reaction to the auditory stimulus under artificial conditions, for while we are accustomed to the transition from darkness to light, even in the abrupt way involved in the experiment, we do not so naturally pass from darkness to sound. For these reasons it seemed best to use only one form of stimulus, and that under nearly as usual conditions as possible.

The figures used with their dimensions, and designated by letter, are as follows: *A*, circle, 100 mm. in diameter. *B*, circle, 100 mm. in diameter, with central portion cut out, leaving a ring 25 mm. in width. *C*, equilateral triangle, 100 mm. a side. *D*, Roman cross, upright 135 mm.; cross piece 100 mm. placed 30 mm. from the top of upright; width of cross piece

and upright, 30 mm. *E*, four-pointed star, 100 mm. in a straight line from opposed angles; perpendicular from each angle to the base of its involved triangle, 35 mm. *F*, Greek cross, 100 mm. cross pieces, 25 mm. incut. *G*, Isosceles triangle, 100 mm. length of equal sides, 90 mm. base. *H*, rectangles 100 mm. in horizontal, but varying from 100 mm. down in perpendicular direction.

There were in all 22 observers selected indifferently from students of all grades of college standing, from the freshman to the senior years. The list also includes two members of the department of philosophy. Apart from the latter, some had never studied psychology and consequently had no previous knowledge of the problem on hand, some were pursuing an elementary and some an advanced course, while one was working on a special problem in the laboratory. In view of the general unanimity in the results, this diversity in the preparation for such work gives to these results an increased significance. Sixteen observers gave only one sitting of an hour, 2 gave two sittings, 1 three, 1 four and 2 gave six sittings. There are recorded for the 22 observers 228 judgments, a little more than 10 introspections on an average for each individual. We also find 25 more judgments for rectangles than for the other figures, the numbers being respectively 126 and 101. But there is a larger number judging other figures than the rectangle — 18 for the former and 12 for the latter. With 3, rectangles only were used; and with 10, the other figures only; while with 9, both kinds were employed. From this it will be seen that in working out the problem no element was neglected or unduly emphasized, and that whatever differences in the way of simplicity or difficulty presented by the figures themselves would be overcome in the process as a whole.

III.

This investigation was begun, as we have said, to throw light upon a very definite and limited problem. With its progress it was found to involve a number of other interesting though related inquiries. From the beginning the series of interests grew, and the observer was questioned on all of them with a view to bring definiteness into his own descriptions of

mental process. The points that came to be more distinctly emphasized will form convenient headings under which to group the material of the introspective notes.

1. *The Temporal Relations of the Illumination and the Perception of Form.* — Waiving for the time being all questions concerning the character and genesis of what may be called illumination-consciousness, the fact to which the testimony points is that there is a psychological time scheme within which it and the resultant perception both fall. There is, that is to say, a clear recognition of the time difference between the consciousness of the stimulus (light) and that from which the stimulus comes (form). The evidence on this point is unequivocal. There is no single case where we had to record a failure to react to the light stimulus as something distinct from the figure which by its means became discernible. Fusion of the two factors in a single experience sometimes occurs, but this is not so common as to throw doubt upon the other case, not more so than a distinct diminution of the time-interval which is also remarked upon. These we regard as variations due to the length of the time exposure, or to the interest of the observer at the moment, and perhaps in the majority of cases both act as coöperating causes to bring about the modification.

Confining ourselves to the main point, S. says: "I got the illumination before I got the figure. I could not have told what the figure was unless I had looked at it," that is exercised discriminating consciousness upon it. There "seemed to be," remarks He., "a difference in time between the light and the figure. I saw the light before I saw the figure." La. got the illumination as "a flash of light, definitely restricted in area, but no definite shape. Did'nt know what the figure was until it was gone." In this Do. agrees. He says, "I got the figure after it was gone. At the instant of the flash did not see the figure." R. "saw the light surface over more of the screen than the rectangle, and only came to the rectangle as a yellow surface gradually and later." K. says, "my feeling for the illumination came before my perception of the object." Ho. "did not see the figure quite so soon as the light. The first sensation was a sort of rectangular light; then I turned my eyes

toward and it was a cross." N.'s experience is almost uniformly described by saying the "figure grew gradually." H. "became aware of the shape or form after the illumination." Although in another experiment he remarks "what I was looking for was the figure," he saw "the illumination first, the figure second." "Quite an appreciable interval," he declares on another occasion, "between the shining of the light and knowing what it is."

We have not included in the above account the testimony of each of the 22 observers because the uniformity in their way of expressing the fact — 'I saw the illumination before I saw the figures' — would not add anything by repetition to the weight of evidence, besides being monotonous reading. I have selected the characteristic descriptions, adding only at this point that all the observers are agreed on this single issue. The experience comes to each in a way similar to that of the several selected witnesses. On the other hand I append *all* the testimony bearing upon the fusion of illumination and form. H. says: "Evenness of illumination and distinctness of contrast are elements in the reaction. Illumination instantaneous and with it came the form." D. "got the illumination so quickly that it and the figure fused in one." Ho., reacting to *B*, says it "seemed to come at once," but adds, "only I did not notice the black center at once." For My. the "figure came without any flash. I saw the figure and that is all I saw." Pa. remarks that "both the light and circle with dark spot in the middle seemed to come at once."

2. *Character of the Psychosis Generated by the Illumination.* — The distinctness in the time when the illumination and the given form are consciously reacted to implies that these are two separate experiences. To put it another way, they are conscious contents differently experienced. That they may be seen to form a more or less inter-related continuum does not effect, at the present, the fact that they stand out clearly from each other with qualitative *differentia*. The one is not experienced in the same way as the other. And if we were inclined to call them both perceptions, the notes make it evident that no more than perception of form is perception of light an instantaneous

experience. Before coming to their awareness of light most of the observers indicate, and describe in various ways, a preliminary or preparatory stage which, on the one hand, is connected with what, at the time, is going on in consciousness, and, on the other, with the meaning of the change which the stimulus initiates. The notes will make these points clear.

Pa. remarks: "When you began talking, my attention was turned to that. It came — whatever *it* was — as a startle. Something happened, something my mind was not prepared for in a way. There was a surprise; an abrupt termination of the series of ideas going on in my mind. During the period of surprise if it continued (*i. e.*, if it had not given place to something else) I could not have told what this thing was, if the question had been asked."[1] Pe. says: "When the flash came I became a blank to what you were saying. There was a change in consciousness, just as if I had been riding in a carriage and had a jolt." He further describes his state as a "sort of nothingness. The factors seem to be the same as in a dream, something you cannot get hold of either at the time or as you look back upon it. After this I see quivering light." L. testifies to an "arrest in consciousness before conscious of light." Do. speaks of being "bewildered for a moment." "Something happened," he again comments, "I do not know what." At another time he "did not know whether it was light or whether you might have said something." He also experiences "a bewilderment, and a different world." Again, it came "like a sort of command—Attention!" If the question had not been asked, R. "would not have known whether it was not altogether imagination. Not sure there was light, only something happened." For Ca. "there was a sudden stopping of the train of thought: stopped because," as he says, "I saw something different." Ko. is 'startled.' "The flash made me stop what I was thinking and saying," says My., and adds "the flash might have been anything, it was something indefinite." Ho. is "conscious of the stream of consciousness taking

[1] However we may characterize this state all the observers agree that it is not a perception. If the state continued no answer to the question what? would be forthcoming.

a new direction when the light comes. What I was saying was interrupted." Re. says "it came in impertinently, didn't want it. I felt it as I would an English word in a (spoken) German sentence." Similarly, N. calls it an 'intrusion.' For Fo. the light came in as a 'speck' which grew into a larger area for a rectangle. He was 'surprised to see the speck.' Co. has both a definite and vague 'arrest of attention,' and also testifies that 'something occurred.' D. is 'brought up suddenly.' Again, has a 'gentle shock as the first impression.' He also describes his experience as a 'distinct call to business.' W. says: "There was just enough to say there was something. My mind was preoccupied. It had to become disengaged to get to this. No recognition of color or shape in this experience. There was a disturbance of some kind. That is why I quit thinking what I was. The disturbance comes first." The same observer says the order of his experience is "there's something there; it's light; then the shape." Then to illustrate what he means he says: "If that were color there, *e. g.*, yellow or blue, I would know there was something there before I knew the color. There would be the possibility of being mistaken about the color.[1] There seems to be a lapse of time before the light reaction set in." H. feels a "breaking off of interest in conversation and 'here's the matter in hand, what is it'? There is psychic arrest and a new psychic attitude initiated with the question."

3. *Growth in the Perception of the Form.* — A distinction is to be made here to clear up the ambiguity of the word perception which sometimes is used to designate a process, and sometimes a product of consciousness. Speaking first (1) of the growth in the psychic process, three stages are to be recognized. (*a*) Sometimes no development takes place beyond the perception of the light; (*b*) sometimes a development which falls short of a perception of the objective form; (*c*) a development which terminates in a true perception, that is, one corresponding to the exposed form. The second (2) topic will con-

[1] It should be remarked that this observer has a particularly well-developed color sense. The significance of the possibility of being mistaken as regards the color, however explained, cannot be put down to defective or inert sense organs.

sider the changes which take place in the objective form itself; what the form has to do with the way in which one consciously comes to a knowledge of it. These points we shall take up in order — we shall also connect the evidence with the particular form under consideration. The form letter, or proportion of sides in the case of rectangles, is placed in brackets for the purpose of distinction. Also, all the examples are brought under review.

(1) *Growth in Perception Regarded as Psychic Process.* —

(*a*) Examples in which perception does not, at best, get beyond the recognition of light. Da. (*C*) feels he "ought to see a figure." "I did see light," he says, "but when I turned to the figure it disappeared." La. (25 × 100) had a feeling of incompleteness — had no particular attributes at first." R. (*F*) says it "simply interrupted the train of thought. Did not see it." D. (50 × 100) declares there was "no form." What shape there was was oblong. More positively in another case (30 × 100) the same observer describes the experience as "too indeffinite; knew nothing of the form." H. (25 × 100) "caught the figure at the lower left-hand half. Gone before I turned my eyes." W. (*D*) asserts, "I have no idea of the shape of that. An indefinite hazy patch of circular light.

(*b*) In which perception falls short of the true objective form.

He. (*F*) saw an "irregular figure, outline not complete, nor the angles clear." For this observer all the angles of the cross were rounded and the form was very much spread out. The outline was broken in the lower right-hand quarter. The same observer has almost the same difficulty with another form (*E*), only the upper right-hand quarter is missing. All the angles are rounded, the sides elongated, and the whole lacks symmetry. Of it he says it was "irregular in outline; not a cross exactly, but a thing with points." Da. (*E*) perceived a "catenary with a portion cut off."[1] With the exception of it being complete Pe., Fl. and Co. each saw it in the same way. Pe. (*F*) saw "different diameters of light, with no regularity of direction."

[1] In the case of Da. something may be allowed to the influence of association. He had been working on calculus before coming to the laboratory. With the other observers it is undoubtedly a case of imperfect development in perceptual process.

La. (50 × 100) "didn't get clear outline of figure. Couldn't say the size of the figure, only that it was a rectangle by the square corners." Do. (*E*) "thought it was the same figure as the previous one (50 × 100)." He did not "remain here (*i. e.*, abide by his first judgment) but came to 'That's something else,' but what it was did not know until," as he says, "I reflected, and then didn't know completely. Don't know whether you would call it a cruciform. Don't know as I ever saw a figure like that." R. (*D*) saw a "rectangle with a little square above the upper end. M. (*F*) "got only the left-hand side and top. The rest was blurred. The figure was like a square which has little squares projecting on the sides of the square." Ho. (*E*) had "some doubt whether it was a four- or five-pointed star, but sure it was a star." Co. called the same figure a "five-pointed star." D. (40 × 100) peceived a "small white semi-circle obliquely up in front of the screen." Reacting to the same rectangle at another time he has "only a vague notion where the bottom should be drawn, but could not reconstruct the rest." H. (45 × 100) got the "illumination from the top. Round shape at bottom. Had to supply the rest of the figure." Of another experiment (90 × 100) he says, "my cognitive act was quite incomplete. I know there was more there to see than I saw. But it was an experience as a whole." W. (25 × 100) "did not react to it as a rectangle at all. If I did not know the series in which it was, I would not have called it a rectangle." The same observer remarks of (*D*), "the nearest to the shape I got is an old-fashioned coffin, wider across the shoulder, narrower toward the head and feet. Nothing sharp or definite about the outline. Seemed pretty bright about the centre, more so than the outer edges." Of (*F*) he says "the first thing was a disc or circular figure, but the last thing I had was a square." (*E*) was described as "something like a pointed concern," that is, only the points of the star were perceived, the entire body was missing. Fo. saw (*F*) as a Maltese cross.

(*c*) In which perception is true to the objective form.

Ma. (*E*) "didn't get the form until the light was gone and I was telling you about it, then I saw it on the screen." For Ho. it "seemed as if the light faded from the centre (*B*) in a round

fashion." Fo. "made a conscious effort to see the light" yet saw the figure to be "round." (*B*) "At first," for Fl. "seemed all light. Black spot came slowly." S. got the same figure in the same way. But W., without comment, described it as a "circle with about a half diameter cut out in the centre." He also "saw the whole figure" when given (65 x 100).

(2). *Growth in Perception Regarded as Psychic Product.*— Ma. remarks that the "light came first; after that the form (*D*), and then the discernment of all parts of the form." "Didn't get the form (*E*)," he says, "until the light was gone and I was telling you about it." Re. saw (*F*) "as a keystone to an arch. It developed into a cross as illumination made edges clearer." "The form (*A*)," for N., "grew, but very rapidly. It seemed to be a round spot and grew in all directions." "Form (*D*) came very gradually; the cross beam was focal in attention and came first." For Fl. (*F*) "seemed to grow from above down." "At first (*B*) seemed all light. The black spot came slowly." Co. (*D*) says the "shape came slowly. Left to imagine part of it. Took all the time of the exposure and more to find out what it was." In coming to the perception of (*C*) he "seemed to see a cylinder vertical with the screen one inch in diameter and six in height. This faded into a triangle." S. (*B*) "saw white ring from the first, the black center coming gradually, although there from the beginning." Also of the same form this observer says "the figure became more distinct after it was gone." Da. was first conscious of "light taking shape. I was trying to make out what the figure (*B*) was." In calling (*C*) a "small illuminated triangle," he confesses there was "quite a bit of ideal construction." Of the same form Pa. experienced a "vague indefinite period in which I was wondering what it was before I could satisfy myself that it was a triangle." L. was "conscious of some sort of effort to determine what the figure (25 x 100) was; a feeling of incompleteness — wanted to get at the meaning of this first experience." Do. needed a "moment's reflection to get the content (50 x 100)." "At first," he says of (*A*), "a kind of bigness. Longer I thought of it, it grew larger. While forming judgment of what it was, it came up to the size of a base ball. It grew about one-fifth." Ho.

remarks ot (*E*), "the light was intense and overspread the black. Only when this faded did I get the correct figure. First saw triangle projecting out on all sides from the main body, but could not say whether I saw the main body at first." Ho. (100 × 100), "got a ray of light. Then went out to the figure and got it." D. (70 × 100) remarks upon "an appreciable time for perception — like when one calls and it is some time before you answer." Similarly of (30 × 100) he says, "a white thing out there — an interval — That's it, is it?" Again the same rectangle was "very slow. Very well aware there was something there before I reacted to the form." "Something nasty," he comments on (40 × 100), "what's that that gives me the nasty feeling? Then it came to mind I ought to get somethiug." H. "didn't see the shape (75 × 100) clearly. An element of pleasure at seeing something. Would describe it, as I am thinking of it now, as (60 × 100). As I reflect, not a bad shape." So of (60 × 100) "it grows in size as I think of it." Of (50 × 100) he says, "I feel I have to supply some lacking detail in the sensation. When this is done I like the form." W. first "got the impression of the thing (25 × 100). When I had that I could not have given a name to it. After the thing was off I could trace the broken side around from the impression. I could draw the picture of it about as it is." "It took a little time to get that shape (*E*). First a blurred patch of light, then had to watch it. I seemed to see the shape grow out of the light as if the light were changing form." He "didn't see the black center at first (*B*). Got a white circular figure. Outside seemed pretty clear cut. This inner spot developed in there. After I got the center, it was very black."

4. *Movements.* — In this section certain physical and physiological changes connected with the initial stages of the experiments are to be mentioned. The limitation of these movements to the first steps of the process is a fact of the introspections. In some form or another they are met with, in one or more cases, in nearly all the observers. Their character will be made plain by the following evidence.

Ho. is "conscious of moving the eyes from the door, but was not quick enough to see anything (*C*)." Ma. experienced

a "strain in the eyes. I was expecting and the eyes seemed to pull out toward the figure (*D*). Relief came with the illumination." M. (*F*) says "I was looking toward my left. I knew something took place. There was a thrill in the trunk and less in the legs. None in the arms." Reacting to (*A*) K.'s "first impulse is to lift the arms and to make a motion with the hands. This impulse the result of the suddenness of the experience. Tendency to take a deep breath at the time." "As soon as light came," he remarks, "there was a drawing up — this was subjective — so as to concentrate to go out to the thing (*C*), and this drawing up was immediately contemporary with the effort to get the outline. The movement of drawing up seemed to begin in the trunk and arms and to go up toward the head as far as the two temples. After the light, there was a dropping back, a relaxation, that is, after I am able to name the figure." "I felt the tendency to raise only in the arms and only on the right side of the eye in which I saw the figure (*D*). There was more of a feeling of spreading out within, as though I was being expanded all over." Ca. (60 × 100) says "the first thing I did was to straighten the head up quickly. Knew it was light after I straightened up." "At the first sight there is a moving of the head and a feeling of trying to see it (60 × 100) better; that is, an accommodation of the eyes to the thing whatever it was. Came to know it through the head and eye movements. Don't know what it is when it first takes place." R. describes (*A*) it as a "startle that calls attention suddenly." "My hands were up to my eyes," says Do. "I put them down with a start to see what had happened. I just jerked them down and my eyes flew wide open." After that he saw the figure (25 × 100). In another experiment (*D*) he says, "I have a slight shock all over me like the effect of thunder when half asleep." Although D. had his eyes on the screen, he says "I had to turn my eyes before I could say it was a figure. It was the light that caused me to turn my eyes to get focussed for the figure (*D*)." He. was "not looking when the figure (*F*) appeared. Thought I moved eyes; guess I was getting ready to see what is was." In coming to the perception of (*A*) he remarks, "I was looking to where I should

expect to see you. There was a jerk of the head with the suddenness of the interruption and more than necessary for the change in the direction of the eyes." S. says "I was looking over the top of the screen. 'It' got me down to where it was, whatever it was that was there (*E*)." When (*C*) was exposed this observer was looking toward the left. "I had to jerk my eyes back. Jerked them back because of the contrast. It was involuntary." (*B*) "Came as a surprise. I opened my eyes wide and changed their direction to where the figure was. I could have given detail without eye movements but not with such definiteness." In connection with both (*F*) and (*A*) Fo. "felt a contraction, squinting, of the eyes." There was "blood rushing to the head" in (20 × 100). Fl. "felt it (*F*) through my whole body. Not a startle," he explains, "but something like lightning." It developed through questioning that this observer, as he says, "likes lightning." "That (*A*) made me flinch, seemed very bright." Co. "heard the switch and strained the eyes for the figure." Under the same conditions, in another case (*F*), he "held the breath." D. says of (40 × 100) "it made me shut my eyes. Very white." "Shut my eyes and involuntary retreat" he remarks in relation to (10 × 100). "Then opened eyes, what is it!" Again (40 × 100) "seemed as a dull, heavy, non-painful electric shock in the eyes. Over in a moment. This shock meant I had an experience *not* objective." "Hindrance in the eyes. It was removed. This changed the center from the eyes to the object (30 × 100)." "Was moving," he says in connection with (40 × 100), "and the experience interrupted the sensation due to change of position. Change struck me in the pit of the stomach." "The whiteness of the preliminary illumination" of (40 × 100) "was dragged down and out toward me." H. feels (45 × 100) "pleasant about the eyes, nostrils, and front part of the brain." The first thing W. felt of (40 × 100) "was a quiver, a shock through me like an electric shock. I felt it but could not tell exactly where it was. Felt it more in the arms and legs than in the trunk. It was on the basis of that that I turned to the light." At first (60 × 100) appeared as a "streak. I turned my eyes," W. observes, "and it seemed as if I was following this band

of light and followed it up. When I struck the figure I stopped because there was something different from the band of light." "Turned my head to it (*A*) before it was gone. Didn't realize any change in turning the head."

5. *Supplementary Items.*—The foregoing detail bears more directly on the problem of this paper, yet it does not exhaust the possibilities of the experiment. Incidentally, either through direct questioning or because certain related features assume greater prominence in some instances, other points of interest have been emphasized, sometimes in one or two cases, sometimes in more, and these are grouped together in this section.

(1) Interest.—D. when questioned whether he liked the given form (30 × 100), replied, "my interest is a scientific interest in the fact." Sometimes an observer gets interested in the conversation or some condition under which it is being carried on so that the experiment is lost sight of. Thus Do. when given (25 × 100), says, "I could picture you talking to me about radium. This something appeared before my eyes and I lost the image of you. The flash seemed to come between my eyes and the image of you. For a second or two there was a blank." Another observer may keep an open path to the purpose in hand while still actively engaged in the conversation. When (*A*) was exposed He. remarked "this other (the illumination, etc.), came in because I knew what we were doing." Interest, again, may be lively in connection with the figure itself. Hence S. can say "this knocked everything else out of consciousness for the time being because I wanted to see it."

(2) Suggestion.—Under this head are grouped the few cases in which some part of the experience suggests a likeness to some other, more or less familiar, object. It is interesting to note that they are connected with the illumination, not with the more concrete, so to say, parts of the experiments. Thus (50 × 100) for D. is suggestive of "summer lightning, warm, reddish pink." (30 × 100), on the other hand, is "cold, blue lightning." (40 × 100) is "like an unusually long flash of lightning; anything may come of it." (40 × 100) is "like a large bony horse." For H. (100 × 100) "opened up as a big white sheet." W. experiences (40 × 100) as "something like

the edge of the sun coming through a cloud — a dull haze," while (75 × 100) has the effect of "distant sheet lightning — seems to spread." Fl. says that (*C*) is, as to hedonic quality, "like awaking in the morning and finding it pleasant weather."

(3) Association. — Unlike the previous group, this one includes the few cases where an apperceptive element is, or seems to be, a distinct factor in determining the judgment. They are influential in determining the perception of form. We have above noted that Da. perceived (*A*) as a catenary, perhaps through associative process. H. feels that he has to "supply some lacking detail in the sensation (50 × 100)" and adds "a distinct apperceptive element." D. confesses that in his perception of (30 × 100) there is a "distinct association of visiting card."

(4) Æsthetics. — Here we place all judgments of preference. They are attached indifferently to the illumination and perception of form. "As soon as I saw the shape (75 × 100)," W. says, "I knew it was pleasing." Co. is "indifferent to the form (*F*). Light was all right, it broke the monotony."[1] Fl. calls (*C*) "very pleasant. The pleasure came the same time as the flash and grew as I saw the form. H. calls (100 × 100) "very nice. It was the whiteness that was pleasing, decided and gratifying." Of (50 × 100) he says, "decidedly illumination is not a pleasant thing." In another instance he "liked the whiteness against the black. Don't know whether like the form (25 × 100)." "The whiteness" of (10 × 100) "very pleasant. Shape not repulsive. Cannot say I like the shape independent of its brightness." (45 × 100) is "very gratifying; visual pleasantness." D. likes (70 × 100) "fairly well. But it is not æsthetic. Like it simply because I can reconstruct it, and so study it at leisure." On the other hand he "cannot reconstruct" (50 × 100), but "likes brightness and form." He doesn't like (40 × 100), it is "too long and too high. Can reconstruct it, but not easily." "To say I liked it (30 × 100) would have to reconstruct it," that is because no algedonic quality developed with the experience. The same form, on

[1] This subject acknowledged to being "uncomfortable in the dark." He "feels a strain to see in the dark."

another occasion, occasioned a "pleasant illumination. Rather a pleasant way of reacting to it. Pleasure fits in with the mood" which he had described as "irresponsible, like a summer afternoon. The figure," he adds, "is not an impediment to the feeling and so indifferent." There was a "surprise in finding this kind of figure (40 × 100) pleasing." Of the same form, again, he was "gratified in getting it out there." He also says of the same rectangle: "Don't like it. Like a great big girl with the frame of a plow boy. Too insistent on self. Nothing modest about it. Relief when it went. Too red."

IV.

We do not attempt, in this paper, to go into the theoretical consideration of the series of problems that the above evidence suggests. There are two reasons for this. In the first place, much of what has come to light, especially with reference to the later, more concrete aspects of the question, only tends to confirm positions that are recognized by contemporary psychologists of various schools. And, in the second place, the more debatable factors, which in general belong to the earlier phases of the experience, could not, in the writer's view, be satisfactorily discussed without going beyond the limitations of the problem as it has been brought forward in this paper. What has been reserved for this section is a more definite enumeration and positive statement of what the positions are to which the testimony points. We shall perform this task with as little dogmatism as possible and without an undue amount of the spirit of controversy. For our interests are primarily descriptive. What theoretical reconstruction is involved in the statement of facts the reader may perform on his own behalf.

1. We are thrown back, in the first place, upon a consideration of *the ground on which the distinction between the illumination-consciousness and form-perception rests*. We have said that these experiences fall within a single time scheme for the several observers. The testimony is that they have now the one and then the other. How are such statements to be interpreted? Have we, for example, a *post factum* analysis, or a contemporary description? So to phrase it raises an inquiry

into the method of the individual observers. Beyond the present problem, the way in which the question is answered will have an important bearing upon the historic controversy between Comte and his opponents with regard to the possibility of introspection as the peculiar instrument of psychological analysis. For our own question we need no more than remark that if the former of the two alternatives be adopted, that this description is due to a *post factum* analysis, a memory image or series of images is substituted for contemporary experience, and that the time order which is the subject of remark relates to it, and not, primarily, to any antecedent occurrence of a psychical order. Beyond the obvious fact that memory images, like the original experiences, are, at the time when consciousness functions in that particular way, *present* data, and therefore not amenable to retrospective analysis, we may point out that by such an admission we are not necessarily bound to regard the evidence in hand as involving the substitution of logical analysis for psychological description. For in the first place the objection unwarrantably transmutes one feature of the conscious content, its retrospective character, into a distinct method, and thereby destroys the content and provides nothing for the method to work upon; and in the second place, confuses the functions of the logical understanding with those of sense. For the psychological question is whether we can get at present experience, whatever its character, in such a way that the temporal relations of that experience are not destroyed. Is our description true; that is, does it correspond to the nature, and the temporal and spatial relations of what is described? And upon this point the testimony of the observers is clear. They do not begin, as we shall note below, with a total to be analyzed, their experience is scrappy. "I could not have told what the figure was unless I had exercised discriminating consciousness, although there was a definite illumination consciousness" is a statement in which all would concur. It is, moreover, a piecemeal experience. As one observer put it, "my *feeling* for the illumination came before my *perception* of the object." But beyond the particular evidence, the experimenter's notes make it clear that, for the observers themselves, it is no ordinary or

usual experience they are describing. They are obviously in *a new relation* to the data of sense. There is, for instance, some hesitancy in giving expression to the first few experiences. And when asked, "why, what's the matter?" to use one form of answer, the reply comes, "oh, it's so funny." And when the novelty has worn off, use does not overcome the strangeness; or, as one observer who had given more than the average amount of time, put it in protest, "I don't see why I don't get the figure when I do the light." I also asked most of the subjects whether they had anticipated what the experience would be, and the unvarying answer was No. The unanimous confession was one of surprise. Such facts as these point unmistakably to something unusual in the way in which the experience develops, and are in support of the contention that introspection, if it is valid, inverts the customary order of procedure, or in other words, lays hold on the original temporal successions of conscious process.

The distinctive psychological character of the evidence is also seen in another closely related question. It may be expressed in an exclamation of the observer already quoted who could not understand why his experience did not correspond with what he knew to be the objective standing of the facts. In the experiments, light is regarded by the subjects as reflection from a given prescribed surface. The reflecting surface is the condition of the generation of the light experience. Why then is the defined surface subordinated in the resultant consciousness to the light which is, as they all think of it, illuminated form? How can the one exist independently of the other? They are surely coincident facts objectively; why do they not come together subjectively? Later we shall have to speak about the character of the elementary consciousness which the illumination initiates; we are concerned here with the fact that there is a distinctively objective phase of the experience in which the light is a real object existing independently of the perceived form. This we are assured of by the evidence of not a few of the observers who, for some reason, were arrested by the mechanism of the experiment, and got a perception of the direction from which the light came. Thus one observer explains, "I never thought of the

light being made over and back of my head; it always seemed out there. It seemed like light shining through paper." Now if this is the normal way of getting the experience, it is difficult to see why the facts as they are described do not have their full psychological value. We cannot square them as readily or at all with a logical interpretation. For in the first place, logical analysis does not normally beget psychological surprise. Surprise is a realized, though not anticipated, contradiction between expectation and experience. Logic has to do with squaring experience with fact. Here facts are discordant with experience which refuses to be squared. And it is just this condition of things that is involved in the psychological order of experience. But, further, the particular analysis to which the evidence points does not warrant us in calling it logical. Logical analysis is performed upon a total complex, and the parts or elements to which it leads are aspects or qualities. We have already said that the observers often fail to get the figure — the substantive fact, but usually come to a consciousness of the light — the adjectival part of the experience logically regarded. Here, then, we have the anomaly — for this point of view — of the parts anteceding the whole; the qualities apprehended without being qualifications of any particular subject. If we were dealing in this evidence with experience of a logical kind we should certainly expect the order reversed; the illumination would not tear itself away from its context so completely as to become a separate fact which may or may not involve any other fact or facts at the same time. But to him who understands the psychology of predication, this independence is not at all surprising; it is the normal method of growth in the case. What is logically last is psychologically first.

Incidentally, we may stop to remark that the two main points already touched upon, that we have a definite time order and a distinct experience of different contents, are not two, but closely related aspects of the given set of facts. When an observer says, "I saw the light before the figure," he not merely calls attention to the succession in the order of experience, but also to the objects which stand to one another in a before-and-after relation. There is opened, that is to say, a way to the

study of the psychology of time, at least so far as time consciousness depends upon and grows out of a real difference in the character of experience-contents. What we mean is that the temporal form in which the descriptions are unanimously thrown would be an enigma if the observers were engaged in ascertaining the relations of differentiated aspects of a single complex. Such analysis, whatever else it might produce, could not give rise to antecedence and succession. Antecedence and succession rest upon an actual difference in the contents of experience. This harmonizes well, as we have seen, with the evidence. Perception of light and of form have a definite time relation because in experience they do not fuse into a single whole, but stand out separate and distinct. We conclude, therefore, that the grounds on which our descriptions rest are not logical, but positively and unequivocally psychological.

2. *Problems Connected with the Psychology of Perception.* — Two facts stand out quite clearly in our study in its bearing upon perception. The first is that perception is a mental process, not an act; and the second that the perceptual content undergoes a growth before it can be accurately defined. These facts, which may be taken as the commonplaces of current psychology, are the starting points of further questions which are still open to further investigation. These questions, all of them, relate to what takes place immediately before, and during the time the mind is coming to, its final position. But before suggesting anything along this line it may be well to point out that while there is a stage in the perceptual process which, from its elementary character, precludes its immediately issuing in a judgment, and which, if it be arrested, nothing could be said concerning the nature of the object which the whole process is intended to determine; yet so far as the process, as process, is concerned it is no different from that which goes on in the further development which is engaged particularly in defining more accurately the relations of the parts within the total complex which is the terminal point of the entire process. If we wish to designate by a distinct terminology what we have above called perceptual process and perceptual product, we may describe the former as

a mental intent and the latter as a mental content. This brings the whole series of changes under a single point of view, and enables us to say that, psychologically, an intent and a content of consciousness are different phases of a continuous growth in mental life. That the material to be organized into higher products is or may be different in these two stages we do not deny. What we are interested in at the present is to suggest that we do not find any reason for interpreting the process of an intent in a different way from the process of a content. The two are distinctly processes of the organization of psychic material into higher conscious products. Now this view of the psychology of perception enables us to raise one or more pertinent inquiries concerning the process which is, as we have said, one throughout. What takes place in the more specialized stage is what takes place in the more generalized stage. The theory of perception must be applicable to the latter as well as to the former.

The whole import of the inquiry thus raised cannot be discussed here. Even all that bears upon it in the present investigation will not be included at this point. We shall only speak of the more obvious or available features of the problem, those that connect with the historic theories of perception. And we shall start from the stage of intention. Some of the observers, it will have been noticed, refer definitely to this. Such remarks as "I felt I ought to see a figure," "I knew what we were doing," "I wanted to see it," "a distinct apperceptive element," etc., unmistakably indicate the influence of intention as a guiding motive in the elementary stages of perception. Furthermore, there can be no doubt that this element, certainly present, as I hold, in all perception, is unduly heightened from the limitations of the investigation. It will be remembered that the observers were told that some form would be displayed on the screen. This would tend, not to introduce any new factor into the normal mental process, but to give a special direction to one of the factors and thereby emphasize its function. In the cases I have cited, and in all others of a similar kind, this over-emphasis has tended to make the intention conscious, at least to the extent of defining beforehand a general class to which the particular form

belongs. This is seen in the remark of one observer with whom, up to that time, I had been experimenting with rectangles. He said: "If I didn't know the series in which it (25×100) was, I would not have called it a rectangle." Also, another remarked: "If I had not been expecting light, I would not have known what it was." But if in these instances the process has become pretty explicit, and in some of their features involve a more complex development than is usually found at this stage, it would not be true to the bulk of the evidence to suppose that intention is devoid of all objective characteristics. Being, as we have said, a stage in the process of perception, intention makes a positive, though not definite, reference to a content which is not identical with itself. The suggestion, therefore, which is unavoidable in the experiment, that some form will be shown, does not impart objectivity into a process which is devoid of it, and the knowledge, general or particular, of the series only hastens the course which runs in normal fashion to the terminal judgment.

The question whether association is involved in our elementary cognitive processes is definitely raised in these experiments. To state the same inquiry another way, we are interested to know what determines, at this stage, the character of our conscious contents. In the examples just quoted, it was, no doubt, preadjustment for rectangles and for light which influenced the final perception. And due allowance in all cases must be made for the influence of the form-concept or light-concept which is controlling throughout the series. Nevertheless the surprising fact is that so few positive examples of association were found. The indications are that a clear interpretative clue to the experience is, in the majority of observers, altogether lacking. And this is true in reference to both the more elementary and developed stage of the process. If, therefore, by association is meant the subsumption of a particular perception under a general idea or class, we do not find that such a procedure is characteristic of elementary psychic process. What we do find is the foundations for such a rational development. We should, consequently, say that association is the outcome of perception and is made possible by it. Stated contrariwise the

way is opened to all the inconsequences of the associationist, besides being contrary to the facts of psychological analysis. But it is not to be presumed that there are no controlling influences or directive suggestions, or that perceptual development cannot be shown to be subject to law. The continuity of the whole process from start to finish, forbids such an interpretation. One observer, for example, calls the first disturbance occasioned by the light and the final perception of the light "two stages in a single process. I do not," he says, "experience them as separated from one another." The problem is how he passes from the first stage which is "a mere impression" of which he can affirm that "'out there' is not the first thing," to the definiteness of experience which enables him to describe it as an "objective process." Whether he can justify the assertion fully or not, we are warranted in affirming that the evidence goes to prove that these conscious processes develop their own suggestions which act as clues to the meaning of experience. In support of this position we may refer to the preponderating number of cases where perception failed to correspond with the actual form exposed, and to the uncertanty that characterizes the detail of the perceptions which were correctly named. For in the latter case we must remember that the name attaches to the schema or general appearance and not to the object as a synthesized complex. That is to say, to correctly name and to correctly perceive an object are not necessarily one and the same thing. For a satisfactory understanding of the perceptual process, therefore, we need a new word to emphasize its non-rational character, and the best for this purpose seems to be the term suggestion. The psychology of suggestion has been developed in connection with so-called abnormal states of mind; but we surely need to bring the results there obtained in line with those obtained by a closer analysis of elementary psychoses. What the mechanism of suggestion may be lies outside the limits of our inquiry, but the character of suggestion is clear. It is a clue developed in normal mental process which becomes controlling in the further development of that process. Its special field of operation is sub-rational consciousness. Further work along this line may profitably be

undertaken, andbelow we shall have something further to say concerning it.[1]

3. *Questions Relating to Undifferentiated Conscious Experience.* — In the immediately preceding topics have been discussed the more concrete phases of elementary psychic process. The more difficult task remains. What can be said concerning the character and method of our most primitive consciousness while still inchoate, and before the first differentiations into definite products appear?

Our resort must, in the first place, be to the evidence already reported. The general fact that there is a period definitely marked off at the beginning of a new conscious process has been established. In attempting to ascertain the character of such a beginning, such a *primum*, the ways in which the observers describe the experience is, before everything, important. The variety of terms used is surprising; but not more so than the fact that they so unanimously indicate a mental, rather than a physiological, condition. Some of the descriptive phrases are in this regard open to question; but, on the most liberal interpretation, not more than a third of the entire list could be claimed to indicate a physiological experience, and all of these might better be considered from a distinctively psychophysiological point of view. Those I have in mind are the following: 'Startle,' 'jolt,' 'brought up suddenly,' 'gentle shock,' 'disturbance,' 'attracted,' 'mere impression,' 'pulsation.' On the other hand, I should claim the following as most fittingly describing a mental experience: 'Surprise,' 'blank,' 'nothingness,' 'psychic arrest,' 'bewildered,' 'something happened,' 'consciousness taking a new direction,' 'different world,' 'interrupted,' 'impertinent interruption,' 'intrusion,' 'direct call to business,' 'the matter in hand,' 'there's something,' 'a command — attention!' 'something else,' 'breaking of interest in the conversation,' 'feeling of oppression.'

It is evident at the outset that not all of these phrases indicate an equally primitive state of mind. Some of them are quite explicit in their reference to what is at the time taking place in

[1] Cf. 'Psychology of Æsthetic Reactions, etc.,' PSYCHOLOGICAL REVIEW, XI., p. 267.

consciousness. In others, while such a reference is undoubtedly present, it is there implicitly. Still a third class is characterized by the absence of any reference at all. Before we go on to study the facts it is important to inquire whether these various classes can be brought under a single point of view. The first two obviously belong together. Are we to consider the third class *sui generis?* It might, of course, be argued that the most unspecialized form of consciousness differs from the more highly specialized in degree only, that, consequently, the second and third groups really belong together, and that all three may be classed together on the principle that they are forms of experience which are determined by the relation that the more recent content sustains to the less recent. This would be, doubtless, a very desirable conclusion to arrive at, and some such position might be indicated if we were able to develop more fully the intentional characteristics of these experiences. But we cannot solve the difficulty, at this stage, by forcing upon the fact's consideration of a theoretical nature which, if true at all, are valid only from the observer's standpoint. What we have to take into account is that for the subjects of these experiments a small, though significant number of experiences do not contain, even implicitly, a reference such as the others unmistakably do. The clue to their classification must be found in the way they are experienced. This is a suggestion which, as old as Hume, was made by one of the subjects at the beginning of the experiments. The second figure to which he reacted 'didn't strike like the other,' which was his first reaction, and the difference consisted in the fact that he 'knew more of what was coming.' This mental contest modifies, though it does not alter the character of the new experience, and it is only in the rare case where this falls into the background so as to be practically inoperative that we get the experience in its primitive form. The relation to other mental content, therefore, of any new conscious experience while important from a developmental point of view, can give us no insight into the character of any elementary and undifferentiated psychosis. If the groups which have been distinguished under this section properly belong here, it is because they 'strike upon the mind' in a similar way, their genesis is

alike. They are forms of conscious experience which are determined by what is not now present to consciousness; they are 'given.' Without exception this is the testimony of the observers. And even in that class where the reference is most explicit to present conscious process, that reference is always to its interruption. The new experience comes; it is never developed out of what is transpiring. If it did, it would not be new. The three classes, therefore, share the same fundamental characteristic of elementary process: they do not develop out of previous processes, but are initiated from without.

If this conclusion is correct we have in it a reason why the physiological factors are not more emphatic or more important. The truth is that we have in them only one set of conditions under which the experience may develop. The distinction between the two main groups into which the cases fall consequently are only of relative importance, now one and now another aspect becoming prominent. Thus one observer can describe the same experience in both physiological and psychological terms. When he says his mind becomes a 'blank,' he refers you to a 'jolt received in a carriage ride.' But immediately in trying to make his meaning clearer he uses the term 'nothingness' as a substitute for 'blank,' and adds "the factors here seem to be the same as in a dream, something you cannot get hold of either at the time or as you look back upon it" — surely a psychological description. In the same way we find that when an observer is impressed with the psychological features of the experience, as for example when the stream of consciousness is interrupted or attention is arrested, there is an immediate reference to something else as happening to account for the change. In either class, therefore, both factors are present, and both classes are alike in their genesis — they are of the nature of an 'intrusion.'

If we wish to gain an insight into the character of the experience, rather than a knowledge of the conditions under which it may arise, we do not find special help in the physiological group over that we may receive from the more distinctly psychological group. It would seem that in every case, as one observer put it, "the process of illumination is a critical thing."

This refers, as we find out from other observers, to the intensity and duration of the stimulus. "A large illumination," says one, "is a challenge to examine the *figure* and cuts out all the first part." That is to say the early developmental processes depend more directly upon objective form than would be possible if the stimulus were of shorter duration, and therefore they come to maturity sooner. This agrees with the testimony of another who is of opinion that "a person gets the experience better if he is not looking quite at the thing and allows it to attract him. Then there seems more to it, and more stages of it." It should be noted that "not to look quite at the thing" has the same effect as shortening the duration of the stimulus. Its influence is similar, namely, to throw one back, so to speak, on the earlier stages of the process and to emphasize these. The problem for the experimenter, therefore, is to find out the appropriate length of exposure for a given observer so as to elicit in the development of the experience just those features which, under ordinary conditions of perception where the stimulus is uninterrupted, would be likely to be hurried over or not consciously felt. The stimulus, however, may be so shortened or may be given under such peculiar conditions that we fail to get anything to our purpose. As an example of the former, one observer "would not have known whether it was not altogether imagination" if a question had not been asked about the experience. Another observer was engaged for the first time in the experiment during a heavy rain storm. The illumination was felt as a flash. "The flash," so the notes read, "made me jump. I didn't know what it was at first, but thonght that it was lightning." Here, of course, we have an example in which the character of the given experience fits in with the general conditions of the moment, and it is difficult to say whether the one or the other, in this case, is primarily suggestive. The complication, however, would warrant us in regarding it as too highly specialized to make it serviceable at this point of the inquiry. But it serves to illustrate the remark that the way the stimulus is given is a crucial factor in the particular manner in which one gets the experience. The experience is the same wherever there is contrast between darkness and light; but the elements are more easily discernible under some conditions than others.

But besides duration, intensity also plays its part in the resulting experience. The number of cases under the section entitled Æsthetics would be considerably lessened if those that depend upon color-contrast were eliminated. Color-contrast, of course, depends, in part, upon the intensity of the illumination. But they would be considerably increased were it not for the number of color illusions that interfered with the development of æsthetic feeling. The factors are too evasive to determine what is exactly controlling in the visual impressions. One observer reacted so unevenly in this regard that the remark was passed upon the fluctuation of the light. In a general way the conclusion is warranted that intensity is directly concerned in developing the coincident processes, and only indirectly with initiating perceptual processes. With this the evidence of an observer seems to agree. He said: "I think a little brighter light would have helped me to like the figure, but the form would not have been my choice at all." The progress of the experience would have been smoother, that is to say, there would have been less hindrance, and the facility would have been pleasing, although the particular figure, under other conditions, would not be æsthetically satisfactory. Hence he remarks: "I should like any figure better for brightening it up, because it would sharpen the edges. Yet this would not change my choice of figures. For example: My choice of figure, even if it were dull in illumination, would be preferred to the figure which, not being my choice, were well illuminated." In other words, intensity in illumination becomes primary in importance in æsthetic judgments, but is secondary in arriving at judgments of perception. This accords with what we saw above, that the longer the duration of the stimulus the shorter the initial stages. This is merely the converse statement of the fact that the lower the intensity, above an ascertainable minimum, the longer these same stages. Duration and intensity work conjointly in inverse ratio to determine the initiation and progress of our elementary psychic processes.

All the cases we have cited imply the fact of change. From this point of view the descriptive phrases may be regarded as attempts to define the supervening conscious process. The

purest forms of the experience are either simply psycho-physiological or æsthetic. Both are alike in having the cognitive factors practically inoperative. Thus, to say that one received a "mere impression" without being able further to characterize the experience, amounts to little more than signifying the fact of change itself. Contrast here is not sufficiently developed to place the new fact upon a background of difference. So far as we can judge from the few cases that are noted, the observer is, in such instances, in a condition of physiological receptiveness, and the new experience is not sufficiently striking to make it stand out from the rest of his sense-feelings. This harmony of the new with the old is also characteristic of those cases of the æsthetic group which lay emphasis upon the mode of reacting. This is brought out distinctly by the observer, who remarked upon the "pleasant way of reacting" to the illumination. The "pleasure fits in with the mood." The absence of demands in these simple æsthetic reactions is emphasized by the further description that the entire experience is "irresponsible, like a summer afternoon." The figure is quite indifferent because, as is remarked, it is "not an impediment to the feeling." In both these cases, the cognitive element, which would emerge in an effort at defining what it is that fits in with the general condition of the moment, is inefficient; the stress is laid upon the process itself. To correlate this with what was said above, we may state that here intention takes precedence of content; a present process is furthered, at least to the extent of lengthening it, by bringing in more suitable material for organization. Such a process, we may remark, grows by what it feeds on; and the lengthening, to which reference was made, means that there is also a growing complexity of structure which ultimately issues, as all intention does when it has progressed far enough, in the definition of a content. There is, that is to say, in all the most elementary phases of consciousness, to adopt the words of Mr. Stout, "an inherent tendency to pass beyond themselves and become something different."

Another, and larger class of cases, shows a more abrupt way of beginning the experience. There is implied a definite mental content which, for reasons special to the individual at the

moment, is practically annulled. An abrupt termination of one process is coincident with an equally abrupt initiation of another. In these cases, contrast is at its maximum. This is true of the following: 'interruption,' 'startled,' 'start,' 'blank,' 'surprise,' 'arrest,' 'consciousness taking a new direction,' 'brought up suddenly.' The explanation of one observer is true of all that the phrases indicate an 'abrupt termination of the series of ideas going on in the mind.' The 'series of ideas' differs with different individuals, sometimes they are ideal contents, sometimes mere sensuous impressions, but the result is the same that as further development in line of them takes place, something different becomes substituted for them. The reasons for the change from one to the other also vary. One 'does not feel comfortable in the dark' and is glad to be relieved; he is ready to react to anything that offers a relief from the tension that controls his feelings. Another is taking part in the conversation in a listless fashion because he 'knows what we are doing,' and holds himself in readiness to attend to it whenever the opportunity is presented. A third becomes absorbed in the preliminaries, and the illumination fairly knocks his present state all to pieces by its very incongruity. One readily develops new interests, and lets anything that is present and familiar go to 'get at the meaning' of the new and undefined. Another is attracted by the different simply because it is different without interest in the character of the thing, or as one observer put it 'because that sort of thing happened' although there was no knowledge of what 'that' was. Sometimes it is a matter of attention growing tired, either when the conversation has been too long or upon an uninteresting topic, and the change is made for 'relief.' Again the introduction of the stimulus may, as it were, develop an entirely new state, as, for example, a 'nasty feeling.' This does not seem to be so rare as the single case in these experiments would indicate. A colleague, Professor F. C. Eno,[1] reports that, under conditions sufficiently exact, he experienced a 'feeling of sickness' in the region of the abdomen under the influence of a sudden auditory stimulus.

These variations, which are not exhaustive, are typical, and

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serve to show the conditions under which a sudden change may be introduced. But they do not aid us to an acquaintance with the experience itself. For this we must look elsewhere. The fact that none of these examples are abortive is significant. They all amount to *something*. They do not, as we have remarked, all lead to a correct perception of the form, and some stop short with the perception of light. But that they develop into some sort of a definable experience indicates that some sort of material capable of growth into cognitive results has become operative. Two questions are pertinent at this point, namely, what kind of material, and how is it received? Upon the basis of these experiments, we cannot give a satisfactory answer to either question. But I may say that the indications are for a modification of the current view which is still too largely under historic influences. Receptivity of impressions, in the sense that some stuff is imported into consciousness to be worked up through various processes into definite products, is only an approximation to the facts. Primitive psychic material does not seem to be so much received from without as developed from within. And this development seems to take place in dependence upon our practical attitudes. It is, in other words, a question *where* the primary reactions are to be placed. The ordinary view makes them take place in response to, and as the result of the arousement of conscious experience. The suggestion we make is that, in what I have called our practical attitude, the reaction is involved in which conscious experience first begins. This is borne out by the variety of reasons that operate in the cases under consideration, reasons that are practical in the broad sense for the observers themselves. It also accounts for the discontinuity that is a feature of these cases, that is to say, for the apparently untraceable beginning of these experiences. These originate, we should say, in the practical solution that is found for the unsolved problem before each observer. There is, on this supposition which cannot be worked out here, no absolute break in conscious process, although in experience there may be the consciousness of difference. Difference there surely is, but it is difference within a conscious *continuum*.

The last class of cases presents a conflict of contents. We may distinguish in the examples a difference due to the emphasis the individual, unintentionally, places upon the conflicting elements. It is to be expected that the observers generally would hold themselves ready to attend to the business to which they were giving themselves, and that thus they would easily drop one end of the conflict in preference for the other. This is the case, for example, when the stimulus comes as 'a sort of command — Attention!' It operates in the first place as a means of calling the mind off from the present occupation to something else which the condition under which it comes make relatively more important. This is true also of the others who refer to the illumination as an 'intrusion,' 'breaking of interest,' 'disturbance.' There is, that is to say, not merely a break in conscious process, but a break which involves a wrench under the operation of a sub-conscious adjustment to just this stimulus. Thus one affirmed his 'mind had to become disengaged' from the present conversation in order to get at the new experience. There is more or less of a struggle on the part of the observer to secure to the intervening factor its rights of precedence. Sometimes the effort is less conspicuous, as when one, in a matter-of-fact way, says 'Here's the matter in hand.' The degree of effort depends, it will be seen, upon how easily revivable is the original suggestion of a light stimulus. In all these cases revivability is sufficiently difficult to introduce a conflict between the incoming and the outgoing processes. Another set of instances indicate that preference is given to the other factor, to the present rather than to the new experience. Hence the illumination, for one, 'came in impertinently.' He 'didn't want it.' The examples which refer to the 'bewilderment' occasioned must be classed here, because there is a 'conflict of contents' with no positive preference for the initiated change. In order to understand these cases, it is important to remark that there is no reason in the character of the new stimulus itself to account for the break between one process and another, or for the precedence that the one takes over the other. This rather is the truth of the matter in the immediately foregoing class when the discontinuity between the two experiences is sud-

den and abrupt. If a new process can develop at the start a sufficient amount of interest, it will more effectively put an end to other conflicting conscious processes, although these may not be devoid of interest, than if it depends upon developing an interest during its course. We have seen a variety of conditions on which interest is conditional in the class where there is an immediate arrest. The stimulus itself becomes interesting in those cases. Is it true that in the present class, in which conflict is prominent, that interest is wanting? On the contrary, interest is a factor here as well, determining, as we have seen, the relative ease or difficulty with which the new process begins. The three classes are not, in their processes, different in this regard. They all illustrate the same fundamental characteristics of elementary psychic life. Only we should say now that they all depend upon the character and amount of interest involved in each particular case. They do not become contents until interest undergoes a development.

On what, then, does this growth of interest depend so that consciousness may become cognitive in form? We may refer most profitably to those instances in which the observer is 'bewildered.' These cases present the factors most nearly in equilibrium. There is experienced the supremacy of now one set and now another set of elements. The 'conflict' is due to the 'want of harmony' of the two. Invariably, the whole is described as a state of 'feeling.' With these facts in mind, I may refer to a case furnished me by my colleague Professor D. R. Major.[1] The connection in which it arose was in trying to ascertain at what age children make an effort to recall past experiences. The subject of the experiment was twenty-eight months old. The material used was geometrical figures of different sizes cut from card-board. "When shown the figures and asked to name them," I quote, "he had no trouble in getting the first part of the combination, *i. e.*, the 'big,' or the 'baby, but the name for the figure frequently failed to appear, and then came the phenomena referred to as an 'effort to recall.' The child when shown the card says 'biguh' and then a pause; then 'biguh' and another pause — the expression of the face

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being either like that of an adult trying to recall a name, or a street number, only not nearly so serious; or, at other times, there were grimaces, stretching the corners of the mouth far apart, showing the teeth, shutting the eyes tightly, etc. I did not keep a careful record of the matter, but I was of the opinion that the desired word rarely came in response to the 'grimace' search for it, and that as a rule it did appear when he stood perfectly still, not a muscle moving, almost suspended breathing and with mouth slightly open." In reporting the case to me verbally Professor Major said the child looked 'bewildered' before the name came, and a 'happy expression' supervened upon the successful termination of his efforts. I do not undertake to say what is the mechanism of the process of recall, but in the statement of facts, as Professor Major gives them, we have the essential phenomena of the elementary processes we are discussing. The consideration of these may throw light upon the other. In all the cases we have studied there is involved the relation of states of feeling to one another. Primarily these are æsthetic with which the comfort or discomfort of the individual is bound up. Progressive differentiation of psychic process has shown that it is under these aspects of feeling that the more clearly cognitive functions emerge. Feeling tends to an average both in kind and amount under given conditions; but in children and in these experiments where we get as near to the simplest forms as possible, it shows itself to be exceedingly unstable. It easily becomes disturbed, passing into one or other of its extremes. This may take place either immediately, as in the second class of cases, or after an appreciable interval, as in the third class. Here I think we have a clue to the phenomena in question. If it is out of our feeling of comfort or discomfort that our definitions of objective facts arise, these objective facts, at this stage of development, can come into relation only through the kinds of feeling they tend to evoke. There are then two general conditions of the life of feeling which are important for psychic differentiation. The first is where the new impulse sets up processes which generate tones of feeling in *agreement* with those already existing, thereby tending to prolong and at the same time to augment them.

Starting as we, supposedly, do from normal we fail to get beyond the immediately present; there is no perception. This is true as we have seen in the psycho-physiological and æsthetic groups. Under the second condition fall the larger number of instances. Here there is *disagreement* of the feeling processes. It may be felt merely, as one observer put it, as a 'feeling of incompleteness,' or in one of the more extreme forms as already noted. This is really the vital group, since it emphasizes the fact of difference which is characteristic of all objective process.

Turning directly to the example, we may I think ascertain the possible elements which enter into a case of bewilderment. In our own experiment there is a balancing back and forth of alternate feeling states due to different sets of elements. There was, the observer remarked 'a want of harmony between what happened and the existing impressions' the 'existing impressions' was a series of visual images. 'What happened,' of course, was light stimulation. It seems to me that in the case of the child referred to by Dr. Major the elements are quite similar. There is an ideal content — he had already named the size of the figure — and a sensuous impression — the form of the presented object. If what we have remarked above is true, in both cases the *impasse* is due to the difficulty each has in bringing the feeling process of the one into agreement with the feeling process of the other. In the case of the child the preference for the successful element of the experiment — the 'biguh' — is manifest in his willingness to let the other go. After the second or third pause he relinguished the effort with obvious pleasure by remarking —'Papa tüll (tell).' Here evidently the remark is in order that one fails of objective knowledge unless the intractable element is reinforced by some practical consideration, and the conflict of feeling thereby maintained. In this case it is the authority of the father who returns the question with an emphatic 'No! — baby.' In the experiments, it is the observer who holds himself to it as part of an existing agreement. Both come to perception in the same way.

What is the way in which feeling returns to its normal condition? Or in other words how do we reach a true cognition? No one can doubt, I think, if he will consider the evidence,

that the most available explanatory factor is found in the large number and variety of movements connected with these elementary phenomena. It is a significant fact, as remarked above, that all the movements were connected with the effort to determine the character of the object which occasioned the disturbance of feeling. Moreover, they are most conspicuously present, and apparently play a more important part, in those cases which involve disagreement in the feeling processes. They are relatively obscure, if not entirely wanting, when there is merely a furtherance of an already existing process. Those processes which involve opposition, radical or modified, we have seen are those out of which perception arises. It would seem, then, that movements such as are present have a distinct cognitive function to serve. The large range of movements may be seen by referring to the various organs that are involved in them. A rough summary gives us the following: Eyes, legs, arms, hands, head, trunk, stomach, nostrils, front part of brain, temples, breath, whole organism. This list becomes more full of meaning when we take into account that not all the observers do the same thing with the same organ. One may, for example, take a deep breath, and another may hold the breath; one may squint with the eyes, and another jerk them wide open; the head may be moved by one, and another have a rushing of blood to the same part, etc. There are also a number of organic reactions which vary with the individual or with the same individual at different times. We note three, described by the terms contraction, relaxation, expansion. Similar phenomena were noted in the case of the child already referred to in his effort to re-cognize a presented object. The coincidence is not merely interesting; it is important as furnishing the clue to the organization of elementary psychic process. Our first sensory data come through these physiological changes which are induced through the disturbance of feeling conditions.

Can anything further be said as to the relation between feeling and the sensations accompanying the various involved movements? To generalize the expression used by Professor Major, what have our 'grimaces' to do with the way we reach knowledge? It may be said, in the first place, that they are

our feelings become objective. They are the immediately available forms of expression of the life of feeling. This will not be so difficult of comprehension when once we have rid ourselves of the false psychology which regards feeling as running its course within a closed circle beginning and ending with the gratification of its own impulses. Feeling, we are warranted in saying, exhibits, no less than conation, 'an inherent tendency to pass beyond itself and become something different.' In the second place, we may remark that these movements are what above I have called our practical attitudes. They are our primitive adjustments to what Principal Lloyd Morgan calls our 'conscious situation.' These adjustments are practical in the sense that they have survival value. This, of course, is only to say they are distinctively feeling attitudes. But, in the third place. survival in psycho-physiological organisms depends ultimately upon the principle of variation. And hence we note that our feeling life undergoes modification and growth through the reactive influence of kinæsthetic sense material originating from the physiological changes which are prompted by it. Through the method of trial and error our reactions become more successful, and these successful relations become more constant when they become reinforced by a stronger feeling-preference.

The following points sum up the results that seem to be warranted by the experimental data furnished in this paper:

1. That our most elementary psychic processes are feeling processes which are not content, but intents of consciousness.

2. That feeling process eventuates in physiological changes which involve movements of the special sense and other organs, that these movements are, on the one hand, the objective side of feeling, and, on the other, practical attitudes toward a present situation, the character of the reaction depending on the agreeableness or disagreeableness of the feeling processes.

3. That these adjustments have psychological importance because of the kinæsthetic sense material which through them become functional.

4. That with the complication of sense data there develop 'suggestions' which operate, under the guidance of feeling, as principles of its organization into definite products or perceptions.

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THE SYNTHETIC FACTOR IN TACTUAL SPACE PERCEPTION.

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One of the dark regions of psychology, where there is an urgent call for more facts, is the process of tactual space perception. There is no reason to doubt that the 'local signs'—the peculiar qualitative marks of a pressure sensation at a given point on the tactual surface which distinguish it from other points and which remain the same for all pressure stimuli affecting the point, are of fundamental importance in this perception of locality on the touch surface. And it is equally certain, *a priori*, that there must be some means of definitely relating these — a framework in which they may be set, in order that they shall constitute a system and that there shall be a relation as of locality. In addition to this it seems reasonably well established that there are two sorts of synthetic factor for these local signs, the inner tactual sensations for the blind, as Heller has shown[1] and the visual image. Many persons with vision, when they try to touch a point which another has just touched, with their eyes closed, as by Weber's second method, find themselves referring the touch they just felt to a visual memory image of the part of the body in question. Some who have experimented and made careful introspections along this line seem to feel assured that this visual factor is the only synthetic factor in the case of any who have normal vision, that the visual image is so much superior to the tactual image afforded

[1] *Phil. Studien*, 12 : 409.

by the inner tactual sensations that it always completely displaces it when present. I cannot cite an author who says this in so many words, but this is the feeling with which I come from a reading of a report as of Miss Washburn.[1] Wundt in his statement in the 'Outlines' is very much more conservative, as also Judd.[2] Wundt says (p. 118): "For many persons the visual images are pushed so far into the background that they cannot be perceived with any certainty even when examined with the greatest attention. The perception of space, in such cases, is perhaps an immediate function of tactual and motor sensations, as for the blind." This seems to me also highly probable. It is further possible that the motor, or better kinæsthetic sensations, are a factor in all tactual space perception, even in persons with a high degree of visual memory — that it plays a less and less conspicuous part as the visual factor grows in importance, but that there is a function of the kinæsthetic sensations in this organic complex which is not to be supplanted by the visual image. When one considers the fundamental importance of these sensations for the blind, and that we all have come through such a non-visual stage in our phylogenesis, it seems still more likely that these kinæsthetic sensations have a perhaps primitive, but none the less essential and by no means usurpable, function in the organization of our tactual space perception. It was with the hope of gathering some facts which would go toward establishing this position that this investigation was undertaken.

It is by no means a thorough canvass of the whole situation, but is rather of the nature of a preliminary report. The attack is made on the influence of the visual factor as shown by the better localization of points touched near prominent landmarks of the visual image, as, *e. g.*, the bounding lines. The relative importance of this factor is judged by the comparison which is afforded between results obtained by Weber's second method (1) with persons having normal vision, under natural conditions of attention with eyes closed, (2) with some of the same persons, with a special effort to recall and use a visual image of the body

[1] *Phil. Studien*, 11: 2.

[2] *Phil. Studien*, 12: 409.

surface in question, (3) with some blind observers, some of whom were congenitally blind and some had sight till three years old. Comparisons are afforded between (1) and (2), and between (1) and (3).

The method of the experiments was as follows: The observer was seated comfortably with the uncovered left forearm resting at ease, volar side up. The experimenter, with millimeter stick and fountain pen then marked eight points on each edge of the arm, the first on each side being on the most conspicuous carpal fold, and each successive one being at a two centimeter interval from the last. Points were marked by a mere touch of the pen. Eight points in the middle of the arm lying midway between these pairs thus marked were also used, making three at each level and twenty-four in all. These middle points were not marked, save in the case of three observers when a special effort at visualization was made, in order not to give the side points an undue advantage, visually, over the middle points by the introduced conditions. With some of the observers the last three toward the elbow, the last six, or the last nine were not experimented on. When ready to begin the experiment the observer sat with closed or averted eyes, and took in his right hand one of two similarly blunt pointed pencils (one millimeter in diameter) handed him by the experimenter. This he carried over to the right side of his body. The experimenter then touched with the other pencil any one of the twenty-four points, making a light but definitely visible indentation and allowing the pencil to rest there about one second. When he removed it the observer tried, as quickly as possible, to touch the same point. He was allowed to move the pencil after touching the arm, provided he should not lift it off the skin. When he halted, the experimenter took the pencil, and with dividers and measure observed the amount and direction of the error. These he recorded on a map of the arm surface, already prepared with the points marked. Thus he proceeded till all the points had been tested. The points were taken in any order, the only control observed, being to avoid stimulating a given point in close succession to a neighboring point, in order to avoid the confusing effect of after-images. Consider-

ing the number of points and the time required to make the record, we feel that the after-images played no part. The experimenter took the pencils together in his hand, and either one was given to the observer when ready for the next test. Only two observers, Ha. and Ru., were given two tests the same day. In most cases successive tests were separated by a week.

Sm., Age 20, F. Natural. Aver. of 7 Series.				Ru., Age 29, F. Natural. Aver. of 5 Series.			
R	*M*	*U*	Av.	*R*	*M*	*U*	Av.
4.6	4.9	5.0	*4.8*	4.1	9.1	5.5	*6.2*
7.8	7.6	5.1	*6.8*	7.8	7.9	8.1	*7.9*
8.1	10.2	8.1	*8.8*	6.5	7.4	7.5	*7.1*
14.6	13.4	8.1	*11.5*	8.2	5.7	10.0	*8.0*
13.4	10.0	9.3	*12.0*	8.3	10.9	8.2	*9.1*
16.2	10.8	7.7	*11.3*				
16.4	15.1	11.2	*12.8*				
12.8	10.3	11.2	*13.0*				
11.7	*10.3*	*8.2*		*7.0*	*8.2*	*7.8*	

Wi., Age 25, M. Natural. Aver. of 6 Series.				Wi. with Visualization. Aver. of 6 Series.			
R	*M*	*U*	Av.	*R*	*M*	*U*	Av.
9.7	7.4	4.3	*7.1*	6.8	5.4	4.5	*5.6*
8.7	7.9	6.9	*7.8*	9.6	8.5	4.7	*7.6*
7.0	7.3	5.3	*6.5*	7.9	5.1	6.6	*6.5*
5.5	10.0	9.0	*8.2*	4.8	7.6	9.6	*7.3*
10.2	11.4	10.1	*10.6*	8.8	6.5	6.6	*7.3*
8.8	14.4	9.8	*11.0*	7.0	6.3	7.0	*6.8*
7.7	17.7	13.8	*13.1*	9.1	8.4	9.1	*8.9*
9.1	8.9	23.2	*13.7*	5.0	5.5	11.2	*7.2*
8.3	*10.6*	*10.3*		*7.4*	*6.7*	*7.4*	

Kn., Age 19 M. Natural. Aver. of 4 Series.				Kn., with Visualization. Aver. of 4 Series.			
R	*M*	*U*	Av.	*R*	*M*	*U*	Av.
5.5	6.7	7.4	*6.5*	5.0	5.5	7.7	*6.1*
11.0	6.4	9.4	*9.0*	7.0	8.2	8.6	*7.9*
10.1	14.1	7.2	*10.5*	6.4	8.0	10.2	*8.2*
11.5	11.2	11.2	*11.3*	9.5	8.9	17.6	*12.0*
7.9	12.7	3.6	*8.1*	9.0	9.2	11.7	*10.0*
15.0	16.2	14.5	*15.2*	12.9	9.4	14.1	*12.1*
9.9	10.9	11.4	*10.7*	11.0	8.1	16.2	*11.8*
12.9	19.9	15.2	*16.0*	16.0	12.2	11.2	*13.1*
10.8	*12.3*	*10.0*		*9.6*	*8.7*	*12.2*	

Pa., Age 19, M. Natural. Aver. of 2 Series.

R	*M*	*U*	Av.
9.5	2.7	3.0	*5.1*
6.5	6.7	2.5	*5.2*
13.5	8.5	7.0	*9.6*
9.0	9.5	10.5	*9.6*
2.0	17.5	9.5	*9.6*
16.5	13.0	8.5	*12.6*
5.2	8.5	16.5	*10.1*
11.0	12.5	16.2	*13.2*
9.1	*9.9*	*9.2*	

Pa. Eyes on arm till touched. Aver. of 2 Series.

R	*M*	*U*	Av.
3.5	2.0	2.0	*2.5*
6.2	6.5	3.0	*5.2*
5.0	14.0	7.0	*8.6*
5.0	6.2	7.7	*6.3*
8.0	4.5	15.5	*9.3*
17.7	8.0	8.0	*11.2*
10.7	10.0	11.5	*10.7*
11.0	6.5	11.5	*9.7*
8.4	*7.2*	*8.3*	

Ha., Age 31, M. Natural. Aver. of 6 Series.

R	*M*	*U*	Av.
8.6	8.1	4.0	*6.9*
5.4	5.3	9.5	*6.7*
10.4	11.5	6.9	*9.6*
7.2	11.1	11.2	*9.8*
9.9	10.9	12.6	*11.1*
8.6	5.7	18.6	*7.6*
11.1	11.7	12.1	*11.6*
16.5	10.5	9.4	*12.1*
9.7	*9.3*	*9.3*	

Mi., Age 16, F. Blind since 2d Week. Aver. of 5 Series.

R	*M*	*U*	Av.
4.7	2.9	3.8	*3.8*
8.0	8.3	5.8	*7.4*
6.6	6.2	9.4	*7.4*
4.8	6.5	6.7	*6.0*
3.4	6.8	6.0	*5.4*
5.6	5.4	4.6	*5.2*
7.0	6.9	5.1	*6.3*
5.7	*6.1*	*5.9*	

Gf., Age 15, F. Blind since 18th Month. Aver. of 4 series.

R	*M*	*U*	Av.
8.6	10.5	5.5	*8.2*
11.7	10.0	10.1	*10.6*
8.2	9.0	9.1	*8.8*
7.7	12.6	11.0	*10.4*
9.5	5.5	12.2	*9.1*
7.2	7.5	9.2	*8.0*
6.7	16.5	8.5	*10.6*
7.2	9.4	17.4	*11.3*
8.3	*10.1*	*10.4*	

Ta., Age 14, F. Blind since 3d Year. Aver. of 6 series.

R	*M*	*U*	Av.
4.3	3.7	6.7	*4.9*
6.3	4.7	6.5	*5.8*
8.7	5.8	8.5	*7.7*
5.7	8.2	9.1	*7.7*
9.2	12.7	7.9	*9.9*
6.2	6.7	10.8	*7.9*
9.2	10.8	11.2	*10.4*
14.7	15.0	14.9	*14.9*
8.0	*8.4*	*9.8*	

Bu., Age 14, F. Blind since 5th Day. Aver. of 6 Series.

R	*M*	*U*	Av.
6.6	6.4	8.4	*7.1*
6.1	8.0	8.8	*7.6*
9.3	10.3	11.6	*10.4*
7.9	11.6	14.1	*11.2*
17.1	17.3	10.9	*15.1*
11.0	14.1	13.2	*12.8*
9.6	11.3	11.2	*10.7*
14.6	10.3	20.3	*15.1*
10.3	*11.2*	*12.3*	

Ev., Age 15, F. Blind since 3d Day. Aver. of 6 Series.

R	*M*	*U*	Av.
7.7	9.2	7.7	*8.2*
7.3	4.8	8.2	*6.8*
7.9	9.7	9.2	*8.9*
5.0	10.0	14.6	*9.9*
12.3	8.6	10.5	*10.5*
11.7	8.7	15.5	*12.0*
9.9	19.8	20.7	*16.8*
11.9	14.2	23.7	*16.6*
9.2	*10.6*	*13.8*	

Co., Age 13, F. Congenitally Blind. Aver. of 6 Series.				Gn., Age 9, M. Blind since 3d Year. Aver. of 6 Series.			
R	*M*	*U*	Av.	*R*	*M*	*U*	Av.
6.7	6.7	7.4	*6.9*	4.1	5.2	7.1	*5.5*
9.3	6.5	10.0	*8.6*	6.5	7.5	7.3	*7.1*
10.7	5.9	6.3	*7.6*	7.5	7.6	13.0	*9.4*
8.7	7.5	6.9	*7.8*	8.1	6.6	10.3	*8.3*
8.7	7.1	11.1	*9.0*	9.2	7.2	14.3	*10.2*
9.4	10.0	11.1	*10.2*	11.2	8.2	15.5	*11.6*
				7.5	8.3	14.1	*10.0*
8.9	*7.3*	*8.7*		*7.7*	*7.2*	*11.7*	

The averages of errors in the tables are arranged just as the points lie on the left arm of the reader when he lays his arm down on the table volar side up. The wrist points are at the top of the table, the radial points to the left, and the ulnal to the right. The number, in each case, is the average, in millimeters, of the errors for the point in question, for the number of experiments given at the top of the table. For instance, in the first table are the average errors of Sm. She was tested seven times on the twenty-four points. The average of the seven errors on the radial point on the carpal fold is 4.6 mm. and the average of seven errors on the ulnal point eight centimeters above the carpal fold is 9.3 mm. These average errors are then further averaged for each of the eight levels at the right side of the table, and for the *R*, *M* and *U* eights at the bottom of the table. Of course the points were not precisely the same in different experiments on the same person, as will be seen from the above description of method. This could have been secured by permanently marking the points. But it was deemed better to avoid such a definite fixing of the visual attention on the experiment. It would at once have introduced an artificial element of considerable importance. As it is, this method is a better means of affording a basis for comparison of errors, vertically and transversely than is the method of areas as used by Pillsbury.[1] The points, considered the same in averaging these results of successive experiments, will all fall in a circle five millimeters in diameter. The purpose of the experiment was not discussed with the observers until after the

[1] *Amer. Journal of Psy.*, 7: 42.

experiments. They were asked where they could localize with most assurance, and by what means they did touch the place. Looking now at the general averages for the *R*, *M* and *U* points in the six observers with normal vision and under natural conditions of attention, for the influence of the visual factor as shown by the larger average error for the *M* points than for the side points, we find larger errors in the middle for four, Kn., Pa., Wi., and Ru. The excess error for *M* over the largest side error, in the case of Kn., 1.5 mm. is perhaps significant. He is a good visualizer. The next best, Pa., with an excess of only 0.7 mm, is by a poor visualizer. Pa. says he never had a visual image of his arm. Ru, with an excess of only 0.4 mm, had no recognizable visual image. The next, Wi., is a good visualizer, but he shows only a small excess of 0.3 mm. Ha. and Sm. are both pretty good visualizers, but had no recognizable visual images. They both give the largest average errors on the *R* side. In this comparison then we have no positive evidence for the influence of the visual factor as making for better localization on the side points. Poorer visualizing faculty is by no means paralleled by decreasing excess of average error in the *M* points. Two fair visualizers give a better average error for *M* than for *R*, and one who seems to have a minimum of visualizing power gives the next to the largest excess for *M* points.

But it may be urged that even in the case of the good visualizers, the visual factor was not called into play, as the artificial character of the experiment set at once a specific touch problem. I do not think this is true, but to set at rest any such objection, and also to see if more evidence of the visual factor could be obtained, I tried Wi., Kn. and Pa. for a number of series each, with a special direction of the attention to the visual image. As Pa. had no recognized visual image, I had him look at his arm while I touched it, and then close his eyes and try to touch the same point. In these cases, as mentioned above, the middle points were marked, as otherwise the side points would have been given just this much more visual emphasis. It is rather significant that each one of these three observers, under these conditions which have been

made especially favorable for more accurate localization on the sides if the visual factor aids in this, gives a smaller average error in the middle than on either side. It cannot be said that this is due to the marking of the middle points, else the marking of the side points and not the middle, in the previous experiments, should have yielded a more significant average error for the middle, for Wi. and Pa. It must be observed, though, that the average error is, in every case smaller, save only the *U* series for Kn. This may possibly be due in some small part to training. But it is difficult to see how practice could manifest such an effect in only four series altogether as there are for Pa. It would seem as though the visual factor thus introduced must be almost entirely responsible for this decrease of the average error. But then the pertinent question remains unanswered, Why do not the regular alignments of the visual image — the side boundaries of the forearm, of which there has been so much more experience and which are more significant altogether than these ink dots — why do these not secure better localization on the side parts? If the better localization on the side parts in these observers, under natural conditions of attention, were due, as was supposed, to the influence of the visual factor, then emphasizing this factor ought to make for a relatively better side localization. Such expectations are reversed. This means either of two things: the visual factor is not prepotent here, or it does not have the effect of influencing for better localization on the sides. Evidently it is a factor; the decreased errors show this. But it is not the important element in localizing on the boundaries of the visual image which some investigators have claimed it to be. Localization on the side of the wrist is better, owing to the peculiar nature of the tactual surface. Bones and tendons are the factors which make for more accurate touch on these parts.

The showing for the part of the visual factor in tactual localization, as given by the larger error in the middle of the forearm, being of such an equivocal character in these normal observers, I was interested to see whether the blind would not give about as significant evidence for a visual factor (?). If the larger errors in the middle are not due to the visual factor, but

are due to accident — a complexity of causes as yet unanalyzed, then the blind might give larger errors in the middle. At the least if we could get results from them similar to those already obtained from observers with normal vision, in respect to the larger errors in the middle of the arm, we should certainly then be able to dismiss the theory that this larger middle error was due to the visual factor. By the courtesy of the Superintendents of the Ohio State School for the Blind, Mr. and Mrs. Smead, I was enabled to experiment on seven pupils in this school. Only one of these was congenitally blind; three lost their sight during the first two weeks of life, and are for this experiment as good as the congenitally blind; one had sight till a year and a half old; and two saw till three years old. They were all experimented upon in the same way as the other observers in the first experiments under *natural* conditions of attention.

We find one of the four, who may surely be considered to have no possibility of a visual factor in tactual localization, Mi., gives the largest average error in the middle, and, when one considers the smallness of the error, it is relatively as much of an excess error in the middle as in any of the normal observers excepting Kn. But this is only one case out of seven, as against four out of six in the seeing observers. Mi. was however, without doubt, the most careful of the blind observers, and the fact, that she gives this larger middle average error, casts serious doubt upon this being due, in observers with normal vision, to the visual image of the part touched.

The general error of the blind decreases from the ulnal (inside) to the radial (outside) of the arm. This progressive increase of the averages as one reads from the *R*, through *M* to *U*, is found in the tables of results presented for Gr., Ta., Bu. and Ev. Greater average error for *U* than for *R* are also seen for Mi. and Gn. Co., the only one who was literally a congenitally blind, gives a very slight excess for *R*. She was not the intellectual peer of any one of the other children. She was more than a year behind the girls of fourteen, though herself thirteen. It should be stated, also, that Gn. was an exceedingly nervous child and that the experiments, as conducted,

were too much of a strain on him to show his best results. He came on with his pencil very quickly after the touch was given, and moved it very little after touching the skin. He said that it got away from him very quickly. This is evidence of a pretty simple dependence upon the touch alone. The whole procedure with him was very like that of a child making his first efforts at penmanship. He lacked finer coördinations, — could not proceed carefully to find the point after touching. Probably, for his degree of attention, the former touch sensation completely vanished when he himself touched his skin. On account of these exceptions, we may say that this progressively larger error toward the ulnal side seems to be characteristic of the blind. There is no such general tendency among the observers with normal vision, nor the counter tendency. Sm. and Ha., the two observers who do not give the largest average error in the middle, do give the reverse, the largest on the *R* side. And with their largest errors in the middle, Kn. tends as these, and Ru. and Wi. tend as the blind.

In regard to the average errors of the different levels, there is no significant difference between the blind and those with normal vision. All show a tendency to best localization on the wrist and progressively larger errors toward the elbow. There are some noticeable exceptions to this, though, in the averages lower than that of the wrist points, for points up to two or four centimeters, as for Wi. (natural) and Ev., in a second low error midway, as at the fifth level in Kn., and in the descent toward the elbow, as in Mi., and in Wi. with vizualization. There is also a tendency to lower the errors on wrist and toward elbow with vizualization. This is the only feature in connection with this comparison of vertical level averages which indicates the functioning of the visual factor, and it is not general. On the whole, there is more difference between the error on the wrist points and those two centimeters above, than between any other levels. This, however, is not due to the better visual image of the wrist, because it is more generally exposed to view. The closer adherence of the skin and the very much more definite local signature which is made possible by the bones and tendons afford an adequate basis for this.

And, on the whole, the blind have it as well as those who see. In fact the progressive increase of error toward the elbow and the exceptions just noted seem, alike, to be more likely to be explained by a reference to general tendencies in the moving member which is doing the localizing, than by reference to the visual factor.

The facts with regard to the *direction* of the errors of localization are difficult to summarize. General tendencies can be made out from the record-maps for the observers, in the order of the above tables of amounts of error, as follows:

Sm.	Great majority of errors *P* (peripheral, or toward the wrist).	Both sides tend in, *i. e.*, error is *U* for radial and *R* for ulnal points.
Wi.	Excess of errors *P*, save in the peripheral-radial third of the field, where it is *C*.	No excess. Both tendencies in all parts.
Ru.	Slight excess of *C*.	Excess of *U*.
Kn.	*C* in central half. *P* in peripheral third.	*R* on ulnal. *U* on radial.
Ha.	*C* except some ulnal points.	*R* on middle and radial. *U* on some ulnal.
Pa.	Almost universal *P*.	Very slight *R* tendency at side points.
Mi.	General tendency to *C*.	Very slight *R* on ulnal and *U* on radial.
Gf.	*C* in excess especially in the peripheral-radial half.	*R* especially in peripheral-radial half.
Ta.	Slight excess of *P* first, then *C* later.	*R* throughout.
Bu.	*P* throughout.	*R* especially on ulnal side.
Ev.	*P* throughout.	*U* on radial and middle; *R* on ulnal.
Co.	Tendency to *P* in central and *C* in peripheral.	*R* throughout, especially clear on ulnal.
Gn.	*P* throughout.	*R* throughout.

The most considerable factor in the majority of the errors was lengthwise of the arm, *P* or *C*. An error of 11 mm. *CR*, for example, would probably be about 10 mm. *C* and 2 mm. *R*. But the analysis afforded in this statement seems to be sufficient for the purposes here in view. It is at once seen that the observers with normal vision, manifest very diverse tendencies, both in the longitudinal and the lateral or transverse errors. Two show an excess of *P*, two of *C*, and the other two *C* on one part and *P* on the other part of the map. This is by no means the general tendency to *P* errors which Pillsbury found. The same observers in side direction of errors give one *R*, one *U*, two *R* on ulnal and *U* on radial, and two no marked side tendency. The lack of general tendency accords in so far with the records of the *amount* of error of these same observers. Of the seven blind observers, there are four *P*, two mixed, and one *C*. And for the transverse error there is yet more accord. All give an excess of *R* errors on the ulnal side. Five of the seven give the same throughout, one giving especially marked *R* errors on the radial side. The other two give *U* errors on the radial side. This marked tendency to *R* errors in these observers seems as though coördinated with the above mentioned tendency which they showed to a progressively better localization on the radial side. Three of these observers show both tendencies. May they not be assumed, provisionally, to be characteristics of the tactual localization reaction in the blind?

This better localization on the radial side can be attributed only to (1) the local signature of this side being more nicely differentiated, or (2) to the inner tactual sensations of the joints of the localizing member, or both. The anatomical structure of the forearm undoubtedly affords a better local signature for the radial side of the volar surface than for the middle or ulnal side. Then too there is basis for better local signature in the natural functioning of the arm. The radial is the front part of the arm. It is naturally carried forward. The forward position is the middle point of the arc of its rotation. There is thus more experience of touch on this side, and experience is a recognized factor in the perfection of local signature. By experience is

here meant not only individual experience but the larger race training. But this perfection of the local signature through experience already involves, as a part of its machinery, the second factor above alluded to as a possible ground of explanation of the smaller errors on the radial side, namely, the inner tactual sensations in the touching member. Tactual space, in the sense of perception of location upon the body surface, comes very largely through the touching of the body by its own moving parts; so that local signature cannot be considered a thing apart from the so-called synthetic tactual factor, except for artificial purposes of analysis. In nature it grows along with the inner local signature of this synthetic factor. But, to return to this second factor as a ground of explanation, of the more accurate localization on the radial side, it seems that the radial side, both in experience in general as above alluded to, and in this special experiment on the volar surface, gets an emphasis as a *line of reference*. The other points are gauged as so far from the corresponding radial point. The inner tactual sensation of tension, which arises when the right arm has touched the radial point of the other arm, may come to pose as the limiting or measuring sensation. This seems plausible, especially from the functional preëminence of the radial part. It is also the only likely ground of explanation I have been able to discover for the predominance of *R* errors in these blind observers who at the same time give their least errors on the radial side. Errors on the other parts are larger and at the same time tend toward this as a line of reference. It emphasizes especially the tensions in the shoulder. If this is true the same should be worked out for the elbow in regard to the *P* and *C* errors. Our results furnish only the suggestion of this possibility in the above mentioned exceptions to the regular increase of error toward the elbow. Wi., in one record, gives the following averages from the wrist up, 8.0, 6.0, 4.3, 7.5, 4.8, 6.6, 5.6 and 4.3. There is a change in the direction of the error at the fourth row. Fourth tends *C* and third tends *P*. So also at the eighth is a tendency to change to *P* again. The carpal points all tend to *C*. From the wrist up these tendencies are successively *C*, *P*, *C*, *P*. It looks like two points of best local-

ization — points of reference as we have called the radial side. Wi. said in this case that the points of the third row (4.3) were the easiest. He often said the points toward the elbow were easy, though he often had an illusion of these being nearer the elbow than they were in fact. Both of these questions should be made the subject of careful investigation with most skillful introspection.

When asked how he proceeded in trying to touch the same point, Kn. said: "It is hard to know where to go if you do not 'strike' at the first trip, especially on the large part of the arm." This indicates a place of service of the visual image. But the visual image is not there to do the work. Of course it is there and functions even when introspection cannot find it. But it cannot give as accurate results as the synthetic tactual factor. Wi. says: 'I have a good visual picture, but localization is not definite in visual terms.' He clearly feels that there is another sort of synthetic factor functioning, though I did not discuss this with him. Ru. says: "I remember the way the place feels. The quickness and accuracy with which I can place the pencil on the arm after the stimulus is given, determines the accuracy of the localization." Ta. (blind) says: 'I am perfectly sure where the spots are, but I can't find them.' And again, 'I know right where you touch me, but I don't know where my arm is.' This calls attention to the inner tactual sensations of the touched member being of importance, as well as those of the touching member. It also indicates the function which the visual image *could* fulfil in her case. It would form the larger framework in which the smaller and vaguer tactual image would find its setting. But the remarks of Wi. and Ru., above, indicate, to my mind, the great importance of this more primitive and vaguer factor; the tactual image is perhaps too dignified a name for it, but it is the immediate framework into which these local signs are set. It is by their places in this that they are signs of locality. I feel that this is the case from my own introspections during this experiment. I can feel where the touch is in a vague way without any visual image, and I cannot feel where it is with a visual image but independent of this which I have called the tactual synthetic factor.

We have evidence of this being used by the other observers with normal vision; and we see that the blind tend to a greater precision (by its exclusive use) on the radial side, whereas the normals (with the visual factor) tend to spread their errors more evenly. The observers with normal vision also show a greater complexity of interworking factors, as would be expected. Greater variety of type of reaction is afforded. This present study has not contributed much to our knowledge of the interworking of these various factors concerned in our tactual space perception. I hope, though, it has, in a measure, restored the synthetic tactual factor to its place, and also called attention to the very rich field that lies here for further investigation both in tactual space perception and in individual psychology.[1]

[1] The MS. of this article was received December 20, 1904.—Ed.

CONSCIOUSNESS AND ITS OBJECT.

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What was considered *selbstverständlich* in previous treatises on psychology, and what even now is set down as something not to be disputed, is that consciousness in subject to immediate awareness; that an immediate intuition of consciousness is possible; and with this as a basis an attempt is made to prove the unity of mind, the falsity of 'atomistic' psychology, or what not, while in philosophy, it is taken at times as a convenient bridge across to reality, some trying even to build up an external world out of consciousness. Now whether or not philosophy depends upon psychology is not here the question, but if philosophy uses among its concepts that of 'consciousness' it must at least take into consideration the knowledge of consciousness as far as psychology is able to give it to us. The trouble with most present philosophy is that it lags behind about fifty years after modern psychology, at the same time declaring with a somewhat lordly air, the complete separation of philosophy and psychology. I think it advisable to examine how far it is possible to have a direct awareness of consciousness, what we mean by a mental state, and to draw therefrom a formula with which we can work in treating of object, consciousness or reality.

I.

The notion that direct awareness of a mental state, of consciousness is possible, arises partially I think, from a confused idea of the possibilities of introspection and introspective analysis. Is it possible to view a mental state as past, or when it is present? Can we hold it up and look at it during its existence, or are we able to examine it as a past mental state? Is it possible for us to be aware of a mental state directly, when such state is consciousness of something else, *e. g.*, an object in the

external world; or of a mental state which is past? The former, *i. e.*, Can we view consciousness as such, directly and immediately, involves the problem of will, self-activity and the self, later to be discussed. The latter inquiry, *i. e.*, Can we view a mental state as past, concerns the problem of introspection and all that it implies.

What is it that we are conscious of in introspection? Of what does consciousness of a past mental state consist?[1] When we say that we are conscious of a past mental state, obviously the past mental state is not the state present. It may seem proper to rest satisfied with saying that we introspectively observe consciousness, and not to go further and inquire what it is which in reality is observed. By saying that we are conscious of a former mental state we tend to delude ourselves with the belief that we have a direct awareness of consciousness. Now the object of introspection is the past mental state. But a past mental state is no mental state; it is a myth; a past mental state has no existence; it has gone forever. In introspection we are not conscious of the consciousness of some object; for that is what is sometimes actually assumed in the expression 'introspection of psychic life.' We cannot be aware of the state as present, for it is past; and as a past state it does not exist; it is not a mental state at all. What we are aware of must therefore be something not mental, *i. e.*, something not wholly psychic, but simply some object as in the case of immediate perception. In introspection I may be conscious of some object to which I take the attitude as past; but I am not aware of a mental state in the past. Consciousness of the past is not past consciousness, and inner and outer perception are both alike, both are occupied with a present object, whatever our attitude be in the matter.[2]

Leaving aside for the moment the question of a self aware of the passing states, a possible objection to the view just presented, is that moments of consciousness are not time moments,

[1] Cf. on this point, Wundt, 'Selbstbeobachtung und innere Wahrnehmung,' *Phil. Stud.*, IV., 1887; James, *Princ.*, 1890, ch. VII., and Stout, *Anal. Psy.*, 1896, Int.

[2] Cf. Münsterberg, *Grundzüge*, I., 1900, II., ch. 7, §3; and Wundt, *Grundzüge*, III., 1903, ch. 15.

but persisting; that instead of a mathematical point of time we may have a 'duration block' of consciousness and as such may become aware of it. "The moments in the schema of time may go on flowing, but the present moment of consciousness may still remain unchanged; nay, it is even conceivable that a present moment of consciousness should fill a whole eternity. The radical difference of these two moments is well illustrated in the popular story of the monk who happened to listen to the song of a bird from paradise for but a single moment and found that meanwhile a thousand years had passed away. The present moment of consciousness does not change with the change of the present time moment; the two are totally different in their nature. Now the moment of consciousness not being a time moment, not being a continual flux as the latter is, may include as well its own consciousness, and thus be a moment of self-consciousness; and as a matter of fact a present moment of self-consciousness does include the knowledge of the present moment of consciousness within the self same present moment."[1]

The argument here presented implies either a self, conscious of its own activity, or the power of introspection to observe a past mental state, the power of consciousness to turn around and take a look at itself as it were, before it vanishes into the past. As concerned at present, I shall examine only whether the difference between the moments of time and moments of consciousness makes any difference in the results thus far obtainnd, that, leaving aside for the present the question of a self aware of its own activity, consciousness must have an object in the present, whether the moments be short or long.

Consciousness at any moment in its persistence, say at its beginning, must be occupied by some object in the present. If we have consciousness of that object, its continuance will not change it into self-consciousness nor allow it to become self-consciousness. The fact of its persistence makes no difference. If another state arise, we do not have consciousness of the consciousness of the object, plus consciousness of the object alone; for the first state must pass away to be succeeded by the following. We may have consciousness of the state as past, plus

[1] Sidis, *Psychology of Suggestion*, 1899, pp. 195–197.

the present object; but this is not self-consciousness. It is simply consciousness of two objects.

If the present moment of consciousness fill eternity we have merely eternal continuance of the consciousness of some object always present. The monk in the story heard simply one long sweet song; of this aware. Had not some other object presented itself, he probably would be hearing it still. If we take the last instant in which the consciousness of the object exists, we have at that moment a moment of consciousness coincident with the time moment, and conditions are then the same as if there were no difference between moments of consciousness and time moments. During the moment of consciousness present, we cannot have another moment of consciousness arising and aware of the first moment. What may take place is an addition to the first object cognized; we would then have a consciousness of two objects, (1) the present object; (2) the past mental state, *i. e.*, no mental state at all, but another object. We would not have consciousness of (1) the object, and (2) the consciousness of this object. Such states would have to be separate moments; they could not be included in the one moment without the implication of an active self. And for such view it makes no difference whether or not conscious moments and time moments are the same or different. If the fallacy does not consist in that of introspection, *i. e.*, awareness of past mental states, then it consists in the supposition of a self aware of its own activity.

In examining therefore whether or not we have a *Selbstthätigkeitsgefühl* or a consciousness feeling its own stream, some misunderstanding arises from a confusion of the facts of be considered with those which are not. The question is not whether or not there is such a thing as self-activity, conation, will or what not; but whether we can be immediately aware of this activity as such. No attempt is made to reduce this activity to body sensations or association complexes. Let the activity be what it will. The point at issue is: Of what are we conscious in moments of activity; what is the content of consciousness in moments of mental exertion other than the so-called cognitive or emotional elements.[1]

[1] See Stout, *Anal. Psych.*, 1896, I., p. 163.

Against the hypothesis that we have a feeling of mental activity *per se*, the 'infinite regress' argument will scarcely hold. As usually given it runs as follows: Consciousness of mental activity would necessitate a double process going on, (1) consciousness of the object, and (2) another consciousness of the first consciousness present, concomitant with the first. If this be so, we would have to have also existing a consciousness (3) aware of consciousness (2) and a consciousness (4) cognizant of consciousness (3) and so on giving us an infinite regress. It may be answered that we need not go back beyond a second consciousness because we can take it as a fact just as opponents of the 'self-awareness' theory stop at a first consciousness. Again if one can use as an argument the infinite regress, the argument of an infinite progress is just as valid in the opposite direction. If a regress is possible to infinity in the former argument, it is just as consistent to suppose such a regress at its end, to turn about and go infinitely forward, till we stop at the object and wipe out consciousness altogether. We would then say not we are 'conscious of an object,' but simply 'object.' Between the infinite regress and the infinite progress, consciousness as such would seem to have little chance of being. How far this is true I shall attempt later to show.

Arguments however may be drawn from the so called physiological substrate of consciousness. Supposing each psychic state to have a physiological concomitant, consciousness of any object would necessitate special cerebral activity; awareness of such consciousness would also presuppose another set of brain cells active when the first are stimulated, and having a concomitant awareness of the first consciousness of the object, which runs along with the other set of cerebral cells. There would then coexist activities in two sets of brain cells, one parallel with an awareness of a psychic state, and this parallel with cerebral activity resulting from stimulation from without. Such a supposition would lead to all sorts of complications, and is moreover unnecessary, the 'parsimony of consciousness' which we believe in precluding any such hypothesis.

Again if we are conscious of mental activity as such, we ought to be conscious of it at all times. If awareness of it does

not always exist, there must be some transition; if so, where is this transition? Now we are not always conscious of this activity, as it is usually understood; moreover we may have great mental activity for a time with small or no sense of effort; small mental activity with great sense of effort; and reversely, there may also be great mental activity with correspondingly intense feelings; likewise in the case of little mental activity. The mental activity as such does not seem to be any measure for the degree of our awareness of it as it ought to be if it has any such concomitant awareness. Fechner's law should hold here as well as in the case of external stimulation.

I shall now attempt to show wherein this feeling of effort, strain, etc., consists. The feelings of strain present in moments of concentrated consciousness have long ago been pointed out by Fechner, and even before him by Tiedemann[1] and analysis was for some time satisfied to consider such feelings of muscular strain and tension as all that are present in this so-called feeling of effort.[2] Wundt however allows such *Spannungsempfindungen* but adds in addition his *Thätigkeitsgefühl* in moments of intense consciousness. Wundt has been variously interpreted; attacks against his theory have been directed chiefly from the standpoint that his 'feeling of activity' and his *Innervationsempfindung* are feelings of pure activity *per se*.[3] In the fifth edition of the Grundzüge, however, Wundt expressly states that the *Innervationsempfindung* can hardly exist as such; that what are so called, are simply motor images; and he proposes to substitute for this much misused term, that of *centrale Bewegungsempfindungen* or *centrale Componenten der inneren Tastempfindungen* (II., p. 31–33); and his *Thätigkeitsgefühl* he describes as composed of a combination of *Erregungs- und Spannungsgefühlen* (III., p. 252). We would then have simply the name; but the fact would be, according to Wundt's interpretation, that consciousness of mental activity as such is not possible.

[1] See Fechner, *El. der Psychophysik*, II., pp. 475–476; Tiedemann, *Untersuchungen über den Menschen*, 1777.

[2] See Ribot, *Psy. of Atten.*, p. 29; Ferrier, *Funct. of the Brain*, p. 464; Lange, 'Zur Theorie d. sinn. Aufm.,' *Phil. Stud.*, IV.; Münsterberg *Beiträge*, heft 2, p. 121.

[3] See Münsterberg, *Grundzüge*, I., p. 529, and James, *Princ.*, II., p. 493.

As components of the feeling of activity, there would then be feelings of strain, tension, etc., whether existing in the skin, and organs, muscles, joints or tendons; and whether peripherally or centrally aroused. Another factor which I think must be considered, is mental fatigue. Granting a physiological basis, any great mental activity would go along with a rapid discharge of cells, causing (1) an increase in the amount of cerebral matter, (2) a greater amount of heat, (3) a growing quantity of waste material, and (4) an increase in the flow of blood. Conditions thus existing would be different from the normal, and the total result would be a certain sensation of heaviness, of tension, of throbbing which would be added to any muscular feelings, etc., present.

In whatever manner it is considered, the feeling of consciousness of its own stream is a myth. When we consider what we mean by effort, this becomes still more evident. Effort put forth presupposes something to which the organism reacts; and something to which a proper adjustment has not yet been attained. A satisfactory attitude is reached only after a number of trials. Effort is possible both physically and mentally and all possible variations exist. The popular superstition that the effort felt is a measure of the work done still clings to the concept with many good people. 'To work by the sweat of one's brow' has become proverbial, yet the sweat and the effort do not necessarily show that any special work has been accomplished. In all cases the sense of effort shows simply that some incompetency transient or permanent, exists in the subject; or that too much has been undertaken; or that the difficulties have been too great; which simply means a lack of power in the subject for that special task. Sense of effort means that energy has been wasted as far as the accomplishment of the task is concerned; that the attitude aimed at has not been realized, or if so, only after a number of unsuccessful attempts.[1]

In mental effort we have a similar alternation of attitudes but in a weaker degree. This agrees with the feeling in many who have put forth physical effort that such sort is much greater

[1] Cf. Dewey, 'Psychology of Effort,' *Phil. Rev.*, 1897.

than mental effort, taken as such. To put it popularly, it is much harder to shovel in a ton of coal, than to think out some scheme of passing it along without shoveling. Some are inclined to place mental effort as greater than physical, because they look at the effects of the former. Mental effort is much more likely to leave after effects which may be injurious; but these have nothing to do with the effort as felt. The view of effort as presented does away with the objection offered to James' 'backstroke' theory by Stout, viz., that if feeling of effort arises from motor innervations, the actions themselves ought to give rise to a more intense sense of effort. As a matter of fact, they do.[1]

Thus far I have endeavored to show that neither by introspection nor by any hypothesis of a consciousness aware of its own stream, can we have any mental state in which consciousness does not have an object; and that in the present. I shall now try to show that the same holds for feeling and emotion.

The James-Lange theory is too well known to need any lengthy exposition. A brief presentation, may, however, be helpful for purposes of discussion. The theory can be set forth no better than as given by James. In his original and striking manner, he says, "My theory . . . is that *the bodily changes follow directly the perception of the existing fact, and that our feeling of the same changes as they occur* is *the emotion*. Common-sense says, we lose our fortunes, are sorry and weep; we meet a bear, are freightened and run; we are insulted by a rival, are angry and strike. The hypothesis here to be defended says that this order of sequence is not correct, that the one mental state is not immediately induced by the other, that the bodily manifestations must first be interposed between, and that the more rational statement is that we feel sorry because we cry, angry because we strike, afraid because we tremble, and not that we cry, strike or tremble, because we are sorry, angry or fearful, as the case may be. Without the bodily states following on the perception, the latter would be purely cognitive in form, pale, colorless, destitute of emotional warmth. We might then see the bear, and judge it best to run, receive the insult

[1] See James, *Princ.*, II. 494 ff. and Stout, *Anal. Psych.*, I., p. 64.

and deem it best to strike, but we should not actually *feel* afraid or angry.

"I now proceed to urge the vital part of my theory, which is this: *If we fancy some strong emotion, and then try to abstract from our consciousness of it the feelings of its bodily symptoms, we find we have nothing left behind*, no 'mind stuff' out of which the emotion can be constituted, and that a cold and neutral state of intellectual perception is all that remains. . . .

"If such theory is true, then each emotion is the resultant of a sum of elements, and each element is caused by a physiological of a sort already well known." [1]

Whatever the emotion be, we have a special form of consciousness, a feeling due to instinctive changes in our body. The statement as given by James in the beginning of his discussion is as he says, a 'crude' presentation of the matter; and one which has led to some criticism of the theory as a whole. When, upon seeing a bear, I feel frightened, such fright may be due to internal changes and muscle adjustments, giving rise to the emotion, and being then followed by further movements as flight, etc. Whatever these changes be, whether in the glands, muscles or internal organs, the emotion is due to them. The visual perception of the bear as such cannot cause fright. I see him in a cage and feed him with peanuts; meet him in the forest and run. The organic changes, in the one case absent, are present in the other.

Of what am I conscious in the latter case? Obviously not of the bear alone. There is also present consciousness of my body in a peculiar state; consciousness of my body in a special way, which I feel and call the emotion of fright. My body reacts in a certain manner; acts as a go-between and determines my future course of action. But the feeling is consciousness and not consciousness of the bear *per se*; nor consciousness in the abstract as some would like to believe. It is consciousness of something, and this something is my body or some portion of it.

[1] James, *Princ.*, II., pp. 449-453; see also Lange, *Ueber Gemüthsbewegungen*, 1887.

Our knowledge of our body in a special way does not usually exist at the time the emotion takes place. The emotion is not localized as here or there; it is simply felt; and connected with some outer object, the bear in the example taken. We do not see it; we do not hear it; but what is more important we do not localize it by the means of the sense of touch. The emotion in most cases is therefore felt, without being felt as here or there; nevertheless it is consciousness of an object, somewhere, and this object is our body. "Attend to an emotion, analyze it and it vanishes." When we try to dissect an emotion, to our mental state become added circumstances other than these originally present. When I try to examine an emotion, there is added the sense of touch, of localized temperature, pressure and the like. I then become distinctly aware of some portion of my body as a localized object, with a tinge of the emotional state. It is not true that the emotion disappears absolutely upon attention to it. There is present consciousness of the body in its disturbed state, plus whatever is added through localization, etc. But the consciousness of my body is then still accompanied by an emotional overtone. What we are conscious of in an emotion is simply our body as an object; we are conscious of our body in a peculiar manner; not visually nor in the usual manner in which we are said to be conscious of any object; but conscious of it without the visual and tactile constituents which almost always are present in our conscious states.

Objecting to the 'crude' statement of James, some have tried to show that what is present is not simply consciousness of the body in a certain state. For example, if emotion is consciousness of diffused organic disturbances, why is not some emotion caused by a cold shower-bath?[1] But surely the organic sensations are not the same in both cases; nor are the constituents alike. Visual, tactile and other elements enter in the latter case, which are not present in an emotion, and which would have no place in a supposedly resulting emotion; nor are there present during the bath any instinctive attitudes or tendencies which might be present in the emotion. Similarly,

[1] Stout, *Man.*, 2d ed., pp. 302–309.

the motor elements resulting from are not organic elements constitutive of the emotion and must not be confused with them. Whether a frightened moorhen runs or dives or hides under a bank makes little difference in his emotional state. The organic and bodily changes have already occurred before the actions succeeding the emotion take place.[1] The emotion need not be confined to consciousness of the leg muscles. Any objections of this kind do not touch the essential features of the theory; that in emotion we have merely consciousness of the body in a special manner.

An attempt is also made to carry emotion beyond the influence of body states by introducing ideational states and higher stages of consciousness. In an emotion there must occur disturbance of mental equilibrium due to furtherance or hindrance of preëxisting conative tendencies; or there may be present ideational excitement in addition to the body changes.[2] But what is disturbance of mental equilibrium but just this consciousness of body changes, sudden and instinctive? Disturbance of mental equilibrium means some change in consciousness, and such change is brought about by instinctive changes and adjustments in the body. Our consciousness, when our mental equilibrium is disturbed, is not consciousness of the disturbing object *per se*. The other constituents helping to form the total stimulation are the body changes described. Disturbance of mental equilibrium is simply another name for consciousness of the body in certain conditions, *i. e.*, for an emotion. In conation, too, there is present simply consciousness of the body in a certain attitude. Furtherance or hindrance of a conative tendency would mean nothing more than furtherance or hindrance of the body attitude, consciousness of such attitude being called conation, and change in it, disturbance of mental equilibrium. In both cases there would be present only consciousness of the body in a special way.

The ideational excitement above mentioned as an addition to the body changes refers specially to revival of ideas of a pleasing kind.[3] One under the influence of opium is said to

[1] See Morgan, *Habit and Instinct*, p. 201.

[2] Stout, *Man.*, 2d ed., p. 308, and Jodl, *Lehrbuch*, 2d ed., II., p. 365.

[3] Jodl, *Lehrbuch*, II., p. 365.

have, in addition to any bodily changes, ideas of a pleasing and exhilarating nature. Here again, however, we have the same thing masquerading under a different name. The emotional quality of any such ideas is due solely to the innervations, tendencies, changes brought about in the body. And such changes added to the coarser effects produced in the body by the opium, give the emotion; again, there being present nothing but consciousness of an object, *i. e.*, the body stimulated in a special way. Whatever name we give to the whole mental state, or to any part of it, all we have as the emotion is consciousness of our body; nothing else.

Finally the objection that emotions are too important in our lives, to be "the 'feel' of bodily attitudes, that have themselves no meaning" can hardly be said to hold. What are we without our bodies, and how is it possible to take any attitude without some feel of them? It is only as the attitudes are felt that we have meaning; the meaning is due to the attitude, and is the consciousness of the attitude.[1] The emotion does not come first and give the 'feel' to the attitude, but it is the attitude which first and last is the most important, the emotion, etc., being simply our awareness of such attitude. Without the possibility of attitudes we would be as nothing, and meaning would be impossible. Even if the emotion is thus 'reduced' to 'sensational' elements this need not disconcert anyone. There is nothing intrinsically wicked in holding this to be the right view, and if this be so, then let it be so. No matter what the emotion be reduced to, it will still be felt as an emotion, and for convenience sake, be so called. The name is the last thing about which to worry.[2]

II.

Many objections are usually raised against the thesis that in the feeling of mental activity and in emotion we are conscious of an object, *i. e.*, our body or some part or parts of it in a particular manner. In pure cognition, however, treating this for convenience sake as separate from emotion and volition, it is more evident that consciousness is concerned with an object, though exactly how, is not a point upon which all agree.

[1] See Dewey, 'The Theory of Emotion,' PSYCHOL. REV., 1894.

[2] Cf. Irons, *Psych. of Ethics*, 1903.

When I see a piece of white paper, I say I am conscious, of, or rather have consciousness of an object. When I see it, touch it or hear it crackle, it is for me an object. We say that consciousness is in some way concerned with it. As a piece of paper it is discriminated from a total field; it stands out; it is 'piece of paper,' at least, whatever else it may be, or whatever other relation it may possess. Considered in itself, it is for me an object. I may concentrate attention upon some writing on it, and this becomes for the time object, whatever else it be, or in whatever relations it stand. The same is true if I look, for example at the letter 'i' which is on such paper, or even at the dot over the 'i.' Each becomes in turn object, whatever else there be present. In the usual manner of expression, I have said, I am conscious of such object. But in the actual moment of presentation, I know nothing about mental activity, consciousness, soul, or what not. I have simply 'object.' This is all that the expression 'consciousness of an object' means.

In the example given, the 'object' is something which can be definitely localized; it exists here or there and has certain spatial relations. I am able to take a more or less definite attitude towards it. Object as commonly understood, carries with it the implication of spatial localization. We need not however restrict the expression to this narrow usage. I may touch something in the dark, without knowing its contour, color, or anything other than its touch. It is none the less for me, an object. So too I may hear a sound, and the sound is for me an object. The very expression 'hear a sound' shows the looseness of our terminology in this particular. I cannot have sound without hearing it, nor can I hear anything but sound. I do not say I touch a tactile sensation, or see a visual impression. I have an object in each case. I touch a bell; I see a bell; and so too I hear a bell when impressions other than auditory are concomitant. But the absence of such accompanying impressions need not drive us to the tautology of, 'I hear a sound.' Rather to be consistent, I hear something, I hear an object, I am conscious of it in a certain way. The same might perhaps be expressed in a more palatable manner, viz., 'I have an auditory impression,' but this would not change the state of affairs

in the least. Of course if 'sound' as commonly used carries with it the implication of objectivity, then, 'to hear a sound' means, 'to hear an object,' *i. e.*, to be conscious of it in a special manner. We would still have an object in such a case. In a similar manner the same may be shown to be true with taste, smell and vision, *per se*, *i. e.*, in each instance we have an object, or to put it as before, we have consciousness of an object in a special manner.

Whenever I am conscious of any part of my body, this becomes for me an object, just like anything else. I touch the piece of white paper, and become conscious of my finger, and of the paper, or of both. If any visual impressions coexist with the tactile, the tendency is to say, that there is some paper present. None the less, there is also present the object finger, whether such be neglected for the time or not. My attitude is more frequently taken towards the object, 'paper' or what not, so I neglect for the time the object finger. If my attitude is taken towards the finger, this becomes the predominating object. I am wounded, for example, in the finger with a pin, and I become conscious of the object finger, whatever else there be present. It seems here that the tactile sensation does double service; that we have awareness of two objects, and only one impression. Closer examination will show that this is not exactly so. If we had only the sense of touch, and nothing else, there would be no possible means of telling whether or not there were two such objects as finger or paper from the impression alone. We would have simply touch, touch, touch; there would be for us simply objects and nothing more to which we would have to adjust our attitude. But when there are coexisting visual or other impressions, these come in to give us the disparateness of the two objects, finger and paper, in the example given. While in the given case the touch is one, and alone would give us simply object, the visual and other impressions give us paper plus finger. In the dark such concomitant impressions are revived to enable us to take an attitude to the object, *e. g.*, paper, as object. If not we have simply some indefinite 'object' to which we react in a certain manner, depending upon our experience. An insect crawling across

my back will not give me consciousness of insect, plus consci ousness of back. All I have is consciousness of back, or at least, as with the child, of some object; I cannot look at my back, I have not seen anything crawl across; the visual impression is lacking; I am aware simply of 'back' or 'object' and take my attitude accordingly.[1] In the young child it is highly probable that such awareness is not definitely localized; that there is not 'back' but simply 'object' or even not this; there may be only a 'something' to which the attitude is taken. Where one finger touches the other at some place, or some other part of the body, I may become conscious of one or two objects, according to the attitude I take; but here we have not one, but may have two impressions, one part of the body stimulating the other. It is not my purpose here to develop any theory of external reality. Whatever be the process we can start with but 'object,' whether such object be our body or something else, and whether such object be called and named 'object,' 'something,' or what not.

A term which might be used instead of 'object' is 'experience' that is, in what are called states of consciousness, we always have an experience. Experience, however, has in it the implication of some more or less active self, soul, or what not, and leans more towards the side of an abstraction in the direction of such supposed self or soul. It seems that 'experience' is one step in the process of abstracting from what we have before us at any moment; that experience is rather an expression likewise, which includes the entire state at any moment. My experience also takes in my will attitude, my feeling and my cognition together; with a reference before and after. It seems better therefore to restrict ourselves to a term which can be used when we deal with these components separately. Object does not mean a scissure from the self or ego, or whatever else we choose to call some such hypothetical active principle. Object always carries with it a certain relation with the self. I have no object unless I am present, and without my being concerned, there can exist for me no object.

[1] Baldwin, *Mental Development*, pp. 119–134; Stout, 'The Genesis of the Cognition of Physical Reality,' *Mind*, 15, 1890.

Object as thus considered is not any supposed *ding-an-sich* Object is simply what is before me at any moment. I may abstract, theorize, and construct a philosophical system giving me a world of external reality apart, or supposedly apart from me (if such a thing be possible); and on the other hand I may build up a theory of consciousness or mind distinct from the *ding-an-sich*. But I must start with the object, for at any present moment this is all I have. It cannot exist without me; I can in no manner conceive any object entirely separate from me. Wherever there is for me an object, there must be some one conscious of it; and wherever one is conscious, he must be conscious of an object.

In so-called higher stages of consciousness, it is not evident at first sight as to what the object of consciousness is; and the temptation is to slide off into easy theorizing and speculation about mental activity, etc. In addition to what has been discussed in this connection, I think it safe to say that higher consciousness deals with images, which sometimes have future or past reference; with words and their meaning; and with relations.

The treatment of consciousness of objects which cannot be distinctly localized will enable us better to understand in what manner so-called images are objects. The popular division considers objects as belonging to a so-called real world, and images as constituting mental creation; the former pointing without and the latter within. But my image is an object, as is a sound. For my image at any moment exists somewhere outside of me in a more or less dim and hazy manner; and like a purely visual or auditory impression it is not definitely localized as here or there. . . . So too, an image is an object in kind like the impression. Abstract from the visual impression all motor and tactile concomitants, and we have an object. It is now and present, and I take an attitude towards it. By a criss-cross check system of impressions, I stamp the one as real, and the other as image. But in the so-called real object, I have constituent aids to the visual image, or impression which are important in determining the attitude taken. The visual image usually has none such, and exists almost purely as visual.

Another popular hypothesis prevails which operates against the calling of images objects. An object is considered as being real; as being constituted by something which has existence outside of consciousness as it were. Now in any moment, in what is actually before me, I know nothing of this; I have simply object. I may build up my system later, and say this object is real, that is not. But this has nothing to do with the actual moment as such. Moreover the image can be considered as having some external and real substratum; only in this latter case, the 'thing' influenced consciousness on some former occasion, and its effects are still present. A real thing, whatever it be, if existing in the former case, can be said just as well to exist in the latter case; the difference being that in the latter its stimulation still has force and still is operating. I bring in the supposed *ding-an-sich* simply to enforce the view that what is called an image may be considered an object as are the objects of consciousness in moments of perception.

A cheap and easy argument against association is that consciousness is teleological; strives towards an end; deals with the future as well as with the past; while association deals only with the past. As a matter of fact, we have neither past nor future consciousness; all I am conscious of exists in the present, and consciousness of the past is not past consciousness, nor have we future consciousness in consciousness of the future. The pastness or futureness of the image is simply the stamp which it has which enables us to take our attitude, and adjust ourselves to it. What we call the pastness of the mental image is simply our attitude towards it, strengthened sometimes by the addition of some word. And such word again, as I shall try to show, has meaning only because of our attitude caused by it. When I try to bring back the experience itself as past, all I have as an immediate awareness is a tendency to turn around and go back. I can get back into the past no further; I am held to the present. The present remains still with me; the past is no more. We are satisfied to stamp a thing with the word 'past,' 'yesterday,' or what not, and act accordingly. Any more we need not, and more we have not. Those peculiar characteristics of the present image, constituted either by its

qualities, by organic sensations, motor tendencies, or by some word connected with it, are all that we are immediately conscious of in a memory image; and these are sufficient to regulate our conduct. But pastness as such we can never experience. I have not attempted fully to analyze what is in the present which determines my attitude to it as past. This has been done rather fully by James, Höffding, Lehman, Münsterberg, Wundt, Bergson, Sollier and others. Whatever it is, it is objective, *i. e.*, there is present some object or objects, either our body or some part of it, together with a dim and indefinite object which we call the image and perhaps also a word; these enable us to guide our action; to take our attitude. The same holds as regards future reference.

The aid given by words in memory is greatly increased in the higher stages of consciousness. In fact, without words, thought could hardly go on. In trains of the most abstract thought, consciousness can be occupied only with words, or word images. The word as spoken, is constituted by impressions of sound, plus motor and tactile impressions of the throat, tongue, mouth, etc. That is to say, we have a combination of impressions, are conscious of a number of objects; and the persistence of such objects in more or less constant relations enables us to use them at all times for the guidance and determination of our attitude. We are prone to look at the word as existing first, and then work back to the impressions. But what we have is simply a number of impressions. These form the word, they are the word. In the word image, likewise, we have simply a skeleton, as it were, of the actual impressions, but enough is present to enable us to adjust ourselves as required. Whether the image is motor, visual or auditory, we are conscious of dim and hazy though constant and persistent objects; though dim and hazy as felt impressions or images, they remain the same or nearly so; they persist in their relations, at different times, and so are of use to us in facilitating consistent action.

Consciousness of the object constituting the word or word image does not give us thought; what is present is simply a number of objects; or, to put it more popularly, a number of

impressions and images. Taken as such, without any concomitant attitude, the prattle of a child is the same as the learned discourse of a philosopher. What differentiates the one from the other is the meaning attached to the one, and lacking in the other. The child's word 'self' is exactly or nearly the same as the philosopher's. There is present simply a number of impressions, motor, auditory or what not. But the meaning given in the latter case makes the word something different from what it is in the former. What we have in the word as such is, therefore, only a number of impressions and images which remain about the same for the same word. And impressions and images, as I have tried to show, are our consciousness of objects.

Words enable us to facilitate our reactions, by acting as 'short cuts,' as it were. Instead of adjusting myself now to this book, now to that, I react simply to 'book' which covers all such cases insofar as the term 'book' is applicable. My attitude is at once determined in its general outline by the word, and naturally so, since it is only after repeated experiences that my attitude towards book has been beaten out, partly by me, partly by the external object. Usually words are connected with certain attitudes which constitute what we call meaning, and, as such, is what is present in thought. In words we are conscious of more or less determinate objects, and in attitude, we are conscious of our body in a specific manner, or simply of images of such attitude or tendencies to such attitude, such images being essentially motor, and in the specific cases concerned constituting the attitude.

There is nothing more mysterious in the word than in the actual object; though both are mysterious enough. In each case we have certain impressions, we take a certain attitude, though in the former our attitude is less filled in and determinate. When I see a chair, I have in the main a visual impression, to which I take a certain attitude. Now for this visual impression I may substitute another, that of the corresponding word, and take a corresponding attitude. The word is an object to which I react just as in the case of the chair. I must make certain allowances in the case of the word, but there

is nothing to prevent my attitude from being essentially the same. I may wish to make my attitude more determinate by looking at a real chair but the essential characteristics of the attitude are there just the same. I read for example, 'The chair is broken,' and I can take my attitude just as well whatever visual impressions I have, whether of the words, or of the corresponding objects. I take a certain attitude. In the case of the word moreover, I can pass it on, tolerably sure to bring about a certain attitude in my reader or hearer, something which I may not be well able to do with the thing itself. Meaning is something which is not necessarily restricted to the word. All objects have meaning, whether such meaning stands out or not.

As in sense impressions in which we pass by well known objects without any special reaction towards them, in which the meaning is not specially emphasized, so too, well known combinations of words may be passed by. There is in the attitude towards both, what might be called a feeling of quiescence. Just as, when I see my well known chair, I do nothing in particular, so in reading an easily understood combination of words, I simply read on. When the combination of words is not well known, I try to substitute others towards which I can take an attitude, which have meaning. When the attitude taken gives this feeling of quiescence, the meaning is understood. It is evident that since the words *per se* is simply a combination of impressions, and the meaning is due to the attitude taken, I may have a word with the impression as such emphasized, or the word with the attitude emphasized. Where words are closely connected with the attitudes toward them, the latter may therefore be emphasized at the expense of the impression or image. That is to say, in reading, we may pass rapidly over the words and dwell chiefly on the meaning. The words as impressions are suppressed, and more place is given to the attitude constituting the meaning. To be more exact, I might say perhaps that consciousness of my attitude constitutes the meaning; but if we consider that no object can exist without consciousness of it, that object has in it the implication of consciousness, it remains the same whether I say 'attitude' or

'consciousness of attitude,' just as 'there is a book before me' equals, 'I am conscious of a book before me.'

The attitude which gives rise to the meaning of an object or word is a rather complex thing, and is not easily analyzed out of the total mental state. We develop it gradually. Before something for the first time, I act towards it in a certain manner, pick it up, perhaps taste it, look at it and so on. Processes like this repeated a number of times will tend to create in me a certain manner of acting in the presence of this thing, whatever it may be. The object becomes stamped, as it were, as a thing to be treated a certain way. My experience with it at different times will tend to make me react a certain way, though any actual reaction be not always made. I am ready, as it were, to act. So when I perceive the name of the thing I take an attitude towards it; and upon repeated experiences with the name, this attitude becomes more and more a mere tendency as compared with its first occurrence. By attitude, as I have before suggested, I mean certain body changes felt either as actual movements, innervations, etc., or as dim images. As far as I can see, the attitude taken where the object or word is well known, is characterized by a feeling of ease or quiescence. By this I do not mean that there is present a pronounced feeling which stands out, as it were, like a signal to tell me all is well. Rather I am in that state in which the object or word does not tend to rouse in me any special attitude; it does not stimulate me to any new effort of adjustment. After repeated experiences with the same object, by a process of analysis, I find that I am in a certain condition; that I have a certain bodily feeling, when I have nothing to fear from the object; when no further reaction is necessary; when the object is known. I do not call it a feeling of indifference, because it does not belong to the pleasure-pain category, being rather the feeling of the body when no special reactive tendencies are present. Such a condition I choose to call a feeling of quiescence. It is marked by the absence of any special feeling and is simply consciousness of the body in a certain condition. Where the word is well known there is present this feeling of quiescence as in the case of the well-known object. I may do nothing in its pres-

ence, but I can do what is necessary. I know I can react properly; I feel I can (such knowledge and feeling being implicit in the bodily state which serves as a sign), and therefore I do nothing. When such certainty is not absolute this attitude becomes more explicit, as a succession either of images, of innervations, or of actual movements, sufficient to result in an understanding of the word, to make the meaning clear, to enable the proper attitude to be taken. If the words themselves cannot give rise to a proper attitude, other words are substituted till a proper process of reaction or tendency in this direction be reached. But such process of explication goes on only so far as is necessary to ensure a sufficiently accurate attitude, *i. e.*, till the meaning is well enough known for the purposes in hand. In meaning, therefore, we do not have any independent 'higher consciousness,' but here, as elsewhere, consciousness is occupied with an object in the present, such object being the body or some part of it.

Thought is concerned wholly with meaning and relations, and it is well to consider in what relational consciousness consists. Expressing symbolically, the fact that one object, a, stands in a certain relation to another object, c, we have:

$$aRc,$$

or, to be more exact, since c might also be in relation to a by the fact of a's relation to it, we would have

$$aRcR'a.$$

Considering 'object' in its wider significance, as including 'image,' a and c may both be objects or images; a may be an image and c not, or c an image and a not. Again, the two may be in consciousness together, or separately, or alternately, and then together; a and c are objects, whether we consider them as images or real objects. The question now is, What is R? Does it depend upon the existence of a and c? Will it fall with the disappearance of the two, and from the latter must be differentiated the question, Must a and c first be before R, and if this is so, can R then exist alone?

Thus far I have tried to show that consciousness must have an object, and I do not see how relational consciousness can be

an exception to the rule. R is obviously not inherent either in a or in c. I may have a alone, or c alone: likewise I may have $aR''cR'''a$. In the former case; a alone is taken in a relational and not in perceptual sense; the latter always having the object in a background, and thus in a relation. But in perception we are not specially conscious of the relation; only when we think over the matter does the relation stand out, and then the consciousness is no longer perceptual but relational. It is also safe to say, that if a and c never existed, R might or might not have existed. Whether, once having existed, it can again take place without a and c depends upon what it is. Whenever I am conscious of a relation as such, that is, whenever I compare, or think about objects and their various connections, all I can find besides awareness of the objects *per se*, is a special attitude which I take towards either or both of them, in their various connections. This attitude must be present before I have the relation. I may compare the length of two sticks, and have besides object a and object c, movements of my eyes passing from one to the other. But such movements are simply the motor concomitants which with the visual impressions go to form my percepts of the sticks, a and c, of the distance between them, and the like. When, however, I find aRc, my attitude is taken; I react towards a, not simply as a, but, as, for example, 'a shorter than b.' If now I wish to make my attitude more explicit, I may take a instead of c, to measure, or to lay across a space, or to throw away, or to react, as the case demands. Once having experienced R, in the example given, 'shorter,' this can stand without the existence of a and c, from which it arose, being now fixed with the word; as a word it has meaning, and may be used in a number of cases. In a relation therefore, I have simply the attitude taken towards a number of objects in certain connections. The formula, aRc, then becomes,

$$aBcB'a.$$

There are present coexisting objects, viz, a, B, and c, in which B is the body in a certain condition; or to put it differently, the consciousness of the relation is only conscious-

ness of the body in a special manner. 'Transition in consciousness' would mean nothing more than this.

III.

Thus far I have been occupied with the seemingly insignificant attempt to show that consciousness, whether in the highest stages of abstract thought, or in the so-called 'feeling of self-activity' is always occupied with an object, either the body or some other object. If then we are careful to explain what is in the field of consciousness at any moment, it is evident that we can explain it just as well by speaking of object in this broad sense, as by using psychological expressions, as image, sense of effort, or what not. All such explanation is individual however, and applies only to the moment in question. To explain a number of different moments we would have to use different expressions in each case, which necessarily would lead to confusion. By generalizing and using such expressions as idea, feeling, etc., any formula or theory will hold good in such general terms for any number of instances. We must however, have, at the bottom of such general terms an object or objects, from which we have abstracted, and from which in crucial instances we can abstract. But we must be careful not to reverse the process.

The usual expression, and one which we have been forced more or less to use in the preceding two sections, because of the current terminology, is that 'consciousness' is concerned with an object. But I can have no object without consciousness; I must always be conscious of an object; the relation which we call 'consciousness' is already implicit in the expression 'object'; 'consciousness' a supposed something, would better be said to be in relation to or aware of the 'real' another supposed something, both of which being abstractions from the object before us.

I may show this somewhat more clearly as follows: "granted a something called consciousness, we find this always in relation to something else, call it 'ding-an-sich' or what you will. If a 'ding-an-sich' be denied, 'consciousness' can just as well be refused. I prefer to speak not of the relation of conscious-

ness to its object, since consciousness is implied in object, but of the relation of consciousness to some real. Neither of these has existence for me as a separate reality, but only as an abstraction. I have simply 'object' at all times. The real and consciousness in certain relations give me 'object.' Expressed symbolically, we would have,

$$O = a'Rx,$$

i. e., the real in relation to consciousness gives us 'object.' But I have no more right to speak of a' as related to x, than of x as related to a' in some manner. We usually think of a one-sided relation, but there is rather an interaction between the two if any. This will give us,

$$O = a'RxR'a',$$

in which as before, a' is the external real, x is consciousness as a real, and R is the relation between them. But any real before reaching this supposed consciousness, must do so through the medium of the body or end organs. Considering simply the relation between the end organs and real, and supposing such relation reciprocal, we have,

$$a'R''b'R'''a',$$

in which b' does not mean body as object (for this has in it the implication of consciousness), but body as real. Such a process takes place for example in instinctive reactions of which I am not conscious. I chose to call this interconnection, the end-organ-process. To be conscious of this, it must be in relation to the supposed x or consciousness as real. This will give us,

$$O = (a'R''b'R'''a')RxR'(a'R''b'R'''a'),$$

i. e., end-organ-process is in relation to consciousness which again is in relation to end-organ-process. But again, such process is mediated by what we call the brain, which we may represent by e', this not signifying brain as body or object but brain as real. We shall have the rather awful formula,

$$\begin{aligned} O = [(a'R''bR'''a')R''''a'R^{\text{v}}(a'R''b'R'''a')]Rx \\ R'[(a'R''b'R'''a')R''''e'R^{\text{v}}(a'R''b'R'''a')] \end{aligned}$$

which means that some real, a' acts on the end organ b' which

reacts on a', resulting in the end-organ-process, which stands in some relation to the brain e'; and e' stands in relation to such process; the whole may be called the end-organ-brain process. The whole is in some relation to x, which again is in relation to the whole. It is of course a question whether in all cases there is a reciprocal relation between end-organ-process and brain before consciousness is possible, or whether we have simply, end-organ-process, related to brain, related to consciousness directly. This would however have no effect on our conclusions. Relation between mind and body means nothing without the action of reals on mind, through body, which real may be the body itself. Whether there be an interval of time between or not does not affect the validity of the formula. For the real may have been in some relation to the end-organ-process, which was in relation to the brain, etc., on a former occasion; the relation however still holds when the occurrence has past, though the relation is now perhaps weakened by the lapse of time, as when we have an image. What the final relation to consciousness is, whether one of parallelism, interactionism, causation or what not, we cannot tell.

The formula as given will do for discussion concerning reality consciousness, and the like. It is seen that in the first place, consciousness is just as much a *ding-an-sich* as the reality; and those who try to connect their experience with reality, or to build up an external world by some connection with consciousness, of which they think they are directly aware, are indulging only in academic juggling with terms which are merely abstractions. Consciousness is simply an arch concept covering all such conceptions as are found by abstraction from the object, in psychology; and any system of reality is likewise the result of abstractions in the other direction. If it be asked how one can possibly construct any psychological or philosophical system when there are present only objects, the answer is simple. We get no nearer the reality in either case but simply add a number of other objects to the one in hand, either for purposes of speculative amusement, or actually to facilitate our adjustments to such objects, as in science, whether psychology, electricity, or what not. We have then instead of O, alone,

$O + O' + O'' + O''' + O''''$, etc. What the reality is we can never know. It is bound up in some manner with another reality, the self, consciousness or what you will, and all we have is the immediate object. This is as near to the reality as we can get. It seems indeed remarkable that men should write learned treatises proving by rather hair-splitting arguments that so and so is the reality, when they have as much of the reality as they can get directly before them. Any system of philosophy which does not in some manner enable us to react more accurately to the object, is simply a mental gymnastic, or an academic word play. And if any system aid us in our adjustments, then what we have is not the reality, but only a working hypothesis as to what it may be, such hypothesis being considered valid as long as it can stand the test of our experiences according to our present knowledge. And such hypothesis consists of a number of other objects, O, O', O'', etc., whether such objects be words, symbols, or what not, which we are able to use for reactive purposes in our present life.

It has been said that 'all the choir of heaven and furniture of the earth' are, due to the research in psychology, nothing but sensations. This is not so, for what we have is first the objects, and then we may abstract, and by a process of reasoning conclude we have psychic states called sensations. If the sensations are considered as such, they come in only as a disagreeable overtone; the objects are still for us, 'the choir of heaven and furniture of the earth.' So, too, God, freedom and immortality, *as real, we can never know in our present state.* What lies beyond the present object, likewise is out of our reach, if it be not object. While we are restricted to the present object, still this does not restrict reality. We are accustomed to let one object, as a word, stand for some other, as a thing. So, too we can think of the thing as standing for some other, whether the real, God or what you will. We do not build up the world from sensations; rather we build up a system of sensations, etc., from the world, by abstraction from the objects before us.

While many will agree to the premises in the first two sections of this paper, it is necessary to pin them down by some

formula, to keep them from wandering into the airy regions of abstraction and speculation. With the above formula before one, it is possible to check the often too exuberant imagination. How the results reached affect the unity of conscious as usually stated, I shall discuss if possible in another paper. I have ended practically with the above formula, because it suits all purposes of discussion. It is seen that in the formula, a', b', etc., are represented as passive lumps; whether the relations result from them, or from some external power is not given. Now, supposing that the reals, a', b', etc., have some spiritual guiding element, we have a formula which I hesitate to put down. Granting a reactive guiding principle to a', b', etc. and representing this by y', z, etc., and considering that the reactive principle of e', is not consciousness but only what it has in virtue of its organic nature as any end organ or bodily part, we have:

$$O=[\{(a'ryr'a')R''(b'r''zr'''b')R'''(a'ryr'a')\}\ R''''(e'r''''r^{\text{v}}e') \\ R^{\text{v}}\{(a'ryr'a')R''(b'r''zr'''b')R'''(a'ryr'a')\}]$$

$$R\,x\,R'$$

$$[\{(a'ryr'a')R''(b'r''zr'''b')R'''(a'ryr'a')\}R''''(e'r''''ur^{\text{v}}e) \\ R^{\text{v}}\{(a'ryr'a')R''(b'r''zr'''b')R'''(a'ryr'a')\}]$$

in which y is the reactive principle of a', z of b', and u of e'. The other terms and relations are the same as before. If we consider that the real a' has in it and implies some guiding spirit, then in the last formula, by further abstraction, the a' should be a'', etc.; or leaving the a' as it is with all its implications, we have the formula on page 246.

In closing I would emphasize the necessity of looking at everything in the third section of the paper as so much abstraction; of remembering that all that is present before us at any moment is an object of some kind or other; and finally, of rejecting any simple formulation as aRx, and the like in dealing with the relation of thing to consciousness. As I have tried to show the relation is much more complex.[1]

[1] The MS. of this article was received February 16, 1905.—Ed.

A MOTOR THEORY OF RHYTHM AND DISCRETE SUCCESSION. I.[1]

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I. Method of Experimentation.[2]

Tables I., II., and V., were prepared from measurements of records taken from up-and-down movements of the hand and arm. The subject held a baton to which a delicate rubber cord was attached. Through guides this cord passed parallel to the axis and close to the surface of the kymograph cylinder. The writing point was attached to the rubber cord itself and a slight torsion was sufficient to keep it pressed against the surface of the drum. The movements were approximately in the same plane, though the records were not materially altered if the baton deviated from the plane. This prescribed path of the baton was the one limitation to the free movement of the subject. The pull of the rubber cord was so slight as to be practically imperceptible when combined with the weight of the baton. By varying the distance of the writing point from the fixed end of the rubber cord, the ratio between the length of the stroke and the length of the record could be varied so that the subject might beat as long a stroke as he pleased. The records taken were all about one fourth to one sixth the amplitude of the actual stroke.

Tables III., IV., VI., VII., were prepared from records taken of the finger tapping on a key, or rubber tambour, and from the movement of the foot in tapping on the floor. The record of the finger was made by a Marey tambour, or an electric marker. In the case of the foot, the heel was kept in position by cleats on the floor, and the foot was made to move up

[1] The MS. of this article was received September 3, 1904.—Ed.

[2] The experimental work on which this paper is based was done in the Beloit Psychological Laboratory recently founded, under the direction of Prof. G. A. Tawney, by the administrators of the Wm. E. Hale Fund.

and down between guides. The stylus was attached to the sole of the shoe and wrote directly on the kymograph drum. The method of recording the beat of the foot directly was satisfactory; the friction of the guides could not be detected, and the record was that of a foot tapping time under normal conditions. The usual precautions in dealing with a double kymograph record were taken.

II. Classification and Definitions.

1. *Classification of the Various Kinds of Rhythm.*

There are many different forms of the rhythm-experience. One hears of the rhythm of prose, of the rhythm of bird music and of animal calls, of the rhythm of walking and rowing, of the rhythm of simple taps or of 'simple sound series,' of the rhythm of verse, and of the rhythm of music. Wide differences are recognized, and some classification based on the causes of these differences is important for a theory of rhythm.

A common classification is based on the 'content' of the rhythm, a convenient and obvious basis of classification. The simple sound series, or a simple movement series, is assumed as the form of 'pure' rhythm, and all other forms of rhythm are deviations due to the nature of the 'content' which modifies the 'pure' rhythm.[1]

The material rhythmized is conceived as an antagonistic force which destroys the regularity and therefore the 'purity' of the rhythm. The more elaborate the 'ideational content,' therefore, the less regular the rhythm and the more must groupings depend on a cause foreign to the rhythm.

By this method rhythms are classified as (1) simple series in which the content is reduced as much as possible and the rhythm is perfectly regular; (2) musical rhythms in which ideational content of a certain type is present; melody and harmony enter in and partly determine the nature of the groupings which the rhythmic forms present, (therefore the rhythm is more irregular, less 'pure' than that of the simple sound series); (3) the

[1] Meumann, E., *Phil. Stud.*, 10, S. 310, etc. Followed by MacDougal, R., Psy. Rev., '02, 9, p. 476.

rhythm of verse in which a content of a much richer and more independent character has a much greater influence in the formation of the various rhythmic groups, and indeed practically dominates the grouping process and the rhythm *per se* is at a very low ebb.

The adherents of this system of classification might have carried the process one step further, and found in prose rhythm the complete domination of the ideational content and the consequent complete elimination of the 'pure' rhythm. But it does not then appear how rhythmic prose is to differ from unrhythmic prose.

This classification of rhythms by their ideational content is worthless. It is based on a hasty generalization of the relation of artistic form to artistic content, and when worked out is simply at variance with the facts. Rhythm as a form in music and verse is in the same case with symmetry in the spatial arts, or logical organization in written composition, or the principles of harmony in musical composition. The material does not war with the form and wrench it from its true proportions. In art works involving rhythm, the rhythmic form is not distorted by the material in which it is embodied. It is rather made by that material, and the most elaborate rhythms would be pointless and hard to grasp without 'content.' Anything that happens to rhythm at the hands of the true artist in his treatment of his material will not affect the 'purity' of the rhythm; it will be just as much a rhythm, in every sense, as the barest set of monotonous sounds that was ever clacked out by a laboratory apparatus. As a matter of fact, the artist observes certain requisites of rhythm which the laboratory worker frequently overlooks.

The classification by modifying content does not represent the observed facts. It is a sheer assumption that regularity is the characteristic of the 'pure' rhythm, but granting for the moment that it is, it is easy to show that the three classes, (1) simple sound series; (2) music; (3) verse, do not show less and less regularity as the theory demands. Owing to the construction of most laboratory apparatus, the simple sound series have usually been objectively regular, but it is easy to prove that very

wide irregularities can be introduced into such a series without destroying the rhythm. Just as great irregularities are possible as in the case of verse with its rich and definite content.[1]

According to the classification by ideational content, musical rhythm should be less regular than the rhythm of the simple sound series. But the fact is that it is only in music that a regular quantitative system has been worked out and applied. It is only in music that delicate and complex rhythms demanding minute and accurately differentiated intervals are possible. The many mechanical devices for producing music are witnesses to an exactness in musical rhythm intolerable elsewhere. Instead of being 'modified' and less 'pure' because of an ideational content, musical rhythms are by far the most regular and the most elaborately wrought in the whole range of rhythmic experience.

As to the regularity of the rhythm of verse, it is true that ordinary verse is read with extreme irregularity. And yet this irregularity is in no wise essential to verse rhythm. Series of nonsense syllables and nonsense verses may be, and usually are read with as great regularity as that of the ordinary song, or of the simple sound series. That the ideational content has little to do with the rhythmic impression is apparent when one listens to the reading of verse in a language of which one does not understand a word. In that case the effect is not unrhythmical. We do not miss the content; we do not feel that the factor which determines the unities and the grouping is lost. The verse has not become mere chaos which must be ordered by an unknown content before it can become a satisfactory rhythm. Not at all; one often notes rhythmic peculiarities and beauties not so apparent in a familiar tongue.

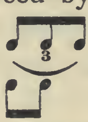
Moreover, it is worth noting in the case of these three divisions, that a form may be transferred, without any change, from one to another without losing its satisfactory rhythmic character. If a simple sound series like the striking of a clock is given a set of melodic intervals, the resulting melody does not show any clash between its content and the 'pure' rhythm in which it moves. One method of piano teaching which has

[1] Stetson, R. H., 'Rhythm and Rhyme,' *Harvard Psy. Stud.*, I, p. 420 ff.

considerable vogue compels the student to translate his composition into a series of clicks at a mechanical keyboard, and thus to master the technical difficulties before it is played at the piano. The clicks of the mechanical keyboard used are precisely a simple sound series, and the rhythms intended for a musical composition with an ideational content have been translated verbatim into this simple sound series. But the rhythm does not therefore suffer, nor is there any feeling of some lost 'principle of unification.'

The content has something to do with the selection of an appropriate form of rhythm, but, that form once selected, it does not enter as a factor into the rhythm.

In place of the untenable classification based on a modifying content, a classification based on the nature of the rhythms apart from their content is possible. The series of simple sounds, the rhythm of verse, the occupation rhythms like walking, the rhythm of prose, and the rhythm of bird songs are all composed of a single series of sensations.

But it is obvious in the case of many musical rhythms, of some tattooes, of patting time for dancers, etc., that the total rhythmic effect is not produced by a single series of beats. Certain rhythmic forms, like , we cannot produce with one hand. At least two processes are working side by side in such cases; there is the accompaniment and the melody, or the time-keeping slow beats against the more elaborately figured primary rhythm; often there are three or more distinct lines of beats playing side by side, now coinciding, and now striking alone. This is no mere matter of mechanical convenience in producing the rhythm; it is heard as two or more processes and so noted in our musical scores. It may be objected that many rhythms which are essentially musical have no apparent accompaniment. For example a melody may be given with or without an accompaniment, without essentially changing its rhythm, which is first and last different from the rhythm of verse. *Apparently* the melody has no accompaniment, but actually the melody has an accompanying rhythm which finds a real em-

bodiment in the organism of the performer and listener. Every melody has a 'time,' a definite 'takt'; it is in 2–4 or 3–4 or some other measure type. This definite underlying beat is the simpler, broader rhythm, always observed, and always felt, without which the rhythm of the melody would become a single rhythm. That performer and listener keep this takt means that it must have some physical embodiment, some corresponding movement, for without such movement, no realization of the two beats to the measure or of the three beats to the measure would be possible.

Rhythms, then, may be classified into two large divisions:

1. Rhythms consisting of a single series of beats — *e. g.*, simple sound series, ticking of clock and metronome, verse, prose rhythm, bird songs, occupation rhythms, etc.

2. Combined or concomitant rhythms — *e. g.*, musical rhythms in all their forms, whether accompanied by changes of pitch or not, dancing.

2. *Meaning of 'a Motor Theory of Rhythm.'*

Laboratory investigations recently published[1] assume the motor explanation. But thus far, little has been done in applying the motor theory to the details of the rhythmic phenomena, and it is by just such a thoroughgoing application that the theory as a principle of explanation must stand or fall. As a general theory, the motor hypothesis needs no defense. Its only competitor was the 'mental activity' theory which is manifestly incapable of explaining the peculiarities of the unit-groups and of the larger groupings. All of the observed facts of rhythm are for it simply arbitrary and unexplained, and its suggestion that content may play a determining part in a rhythm is worse than useless.

When one says that rhythm consists of a series of sensations of movement, or, of a series of sensations of movement in which other sensations (sound, sight, touch) occur precisely as if they were produced by that movement, and that the rhythmic group has the unity of a coördinated action, it is important to know in

[1] Wallin, J. E., *Stud. Yale Psy. Lab.*, '01, 9, p. 130; MacDougal, R., Psy. Rev., '02, 9, 464 and ff.; Miner, J. B., Psy. Rev.,'03, Mon. Sup., 5, no. 4, p. 34.

just what sense the word 'consists' is used. It is evident that the mere presence of the movements which might be the basis of rhythmic experience does not imply the perception of that rhythm. The muscular apparatus whereby the sensational basis of rhythm is produced, is developed in the lower animals. Many of their actions are rhythmical, and the parrot probably reproduces rhythmic forms, but no one would credit the animals with a sense of rhythm. The trotting of a horse produces a vigorous rhythm, but one has only to listen to a well-matched team veering slowly into perfect unison, and veering just as gradually out again, to realize that their trotting is rhythmical, but that they have no sense of rhythm. The simplest suggestion would be a 'center' which combines the motor sensations to a rhythmic perception or a rhythmic emotion. But the multiplication of 'centers' never simplified a problem; it is much more nearly in line with what we know of coördination to assume that rhythm is simply a special form of the ordinary coördination of movement-experience. The Wundtians, who insist that rhythm is primarily an affective experience, can always treat the movement basis of rhythm as Stumpf[1] has treated the Lange-James theory of the emotions, insisting that there is an antecedent central process involved.

The question whether the affective aspect of the rhythm is the essential aspect is really part of a much larger question. If all forms of artistic synthesis, and indeed of any unity, are affective in character, then rhythm is to be so classed. If symmetry, and the metaphysical demand that the world be one, are primarily felt and not perceived, then rhythm is felt and not perceived. The same concrete rhythm may have at different times all shades of affective coloring from pleasant to disagreeable, and must pass the indifference point. Any series of acts is capable of just such changes in affective coloring. To the writer, the simplest form of the rhythm-experience seems simply a perception of a peculiar type of likeness and repetition in a movement-series. Whether or not it is primary no one can deny the importance of the emotional in rhythmic experience.

[1] Stumpf, C., *Zeitschr. f. Psy. u. Phys. d. S. org.*, '99, 21, s. 64–5.

It remains to consider in view of the motor interpretation the distinction between rhythms when merely perceived and when produced by the subject.

The movements involved in the production of a rhythm are always at hand as a basis for the experience of the produced rhythm. But one has as vivid and satisfactory a sense of rhythm when one merely *hears* (or sees, or feels) the series. Where are the movements at the basis of such a 'sensory' rhythmic experience? The body is provided with muscles capable of producing rapid and varied movements not visible to ordinary observation; among these one looks most naturally to those organs which have to do with the production of rhythm. Many musicians keep the takt by tapping the foot, and strains of the muscles of the leg often constitute the silent rhythmic response. Others tend to move the head or the trunk in time; careful observation of a concert audience will show both these types of motor rhythmization. But the most important natural rhythm-producing apparatus is the vocal apparatus. Musicians are frequently trained to count, and suppressed counting is frequent. The tongue is extremely mobile, and the muscles of respiration play a frequent part in rhythmization. The writer finds rapid series rhythmized by slight movements of the muscles of the tongue and perhaps of the throat, in conjunction with the expiratory muscles which mark the main accents. Every rhythm is dynamic; it consists of *actual movements*. It is not necessary that joints be involved, but changes in muscular conditions which stand in consciousness as movements are essential to any rhythm, whether 'perceived' or 'produced.'

In developing a motor theory of rhythm there are certain principles of explanation that are barred. They have frequently found a place in discussions assuming the motor basis, but they will not play any part in the present attempt at a motor explanation.

Analogies drawn from space do not help. A pause is frequently represented as a dividing space; and often the pause is supposed to separate, and therefore to unify the groups.[1] But unless motor changes can be shown to take place during the pause, the pause has no significance.

[1] Wallin, *loc. cit.*, p. 92; Miyaki, I., *Stud. Yale Psy. Lab.*, '02, 10, pp. 16 ff.

Nor does the logical supposition that as single beats are combined into unit-groups, so unit-groups may be combined into phrases, phrases into periods, etc., constitute a legitimate method of explaining the larger groupings of rhythms (Wundt and Meumann). As will be seen later, the nature of the larger groups is entirely different from the nature of the unit-groups.

Judgments of temporal equality or inequality play no part in the rhythm experience.[1] The time judgment is much too vague to determine rhythmic intervals, and accurate judgments of time founded on rhythms are secondary and derived.

Explanations based on physiological rhythms, such as that of the heart,[2] or of a supposed rhythm of the nervous discharge,[3] can avail nothing. Like the 'mental activity,' such explanations fail to account for the wide variations in tempo, and for all the peculiar facts of the unit-group and of the larger groups.

Imagery, or 'central motor discharges,'[4] or 'mental beats'[5] have often been invoked as possible vehicles for a rhythm. Imagery undoubtedly does play an auxiliary part in any rhythm, but that it can actually constitute a rhythm seems more than doubtful. A theory of rhythm in which the essential process is phrased in strictly peripheral terms is safe from the vagueness and uncertainty of the concept of mental imagery. Such a theory could be easily translated into terms of 'central motor discharges'—if their existence should be demonstrated, and rhythms of that type detected.

III. Single Rhythms.

1. *Nature of the Movement-Cycle of Any Rhythm.*

A movement may be perfectly regular, uniform, and recurrent and yet not give the impression of rhythm. If one moves the hand or arm in a circle, the hand may be made to pass a point in a circle much oftener per second that the tempo of the slower rhythms requires, and yet there will be no feeling of rhythm

[1] Squire, Mrs. C. R. (following Ettlinger), *Am. J. Psy.*, '00, 12, p. 541.
[2] Hallock, Miss M., *Pop. Sci. Monthly*, '03, 63, Sept., p. 425.
[3] MacDougal, R., *loc. cit.*
[4] Miner, *loc. cit.*, p. 33.
[5] Wallin, *loc. cit.*, p. 130.

so long as the hand moves uniformly and in a circle. In order to become rhythmic in the psychological sense, the following change in the movement is necessary: The path of the hand must be elongated to an ellipse; the velocity of the movement in a part of the orbit must be much faster than in the rest of the orbit; just as the hand comes to the end of the arc through which it passes with increased velocity, there is a feeling of tension, of muscular strain; at this point the movement is retarded, almost stopped; then the hand goes on more slowly until it reaches the arc of increased velocity. The rapid movement through the arc of velocity and the sudden feeling of strain and retarding at the end of this rapid movement constitute the beat. In consciousness they represent one event, and a series of such events connected in such a movement-cycle may be said provisionally to constitute a rhythm. Every rhythmic beat is a *blow*. The origin of rhythm, as Bücher has suggested, was in forms of concerted work which required blow on blow. That is possibly the genetic reason why the beat, the blow, is the primary thing in the rhythm-consciousness. In all forms of activity where a rhythm is required, the stroke, the blow, the impact, is the thing; all the rest is but connection and preparation. The movement in striking a quick blow or in beating a rhythm may be represented by A and B in the movement curve of Fig. 5 in which A represents the lifting of the member and B the rapid blow.

There is, then, a radical difference between the two phases of the rhythm-movement. The first great difference lies in the velocity of the two parts of the movement. In B of Table I., are given the velocities of the two phases of the movement in the case of three subjects beating rhythms as rapidly as possible. The velocity of the beat stroke is always two or three times greater than the velocity of the back-stroke, and the total relaxion-phase is always at least three times as long as the contraction-phase. A second difference between the two phases lies in the control which the subject has over the moving member; once the beat stroke is started, like a released spring the limb flies to the end of the stroke, and is in no wise subject to regulation; during the back-stroke on the over hand, the movement

Table I.

Velocity of the Down-stroke and of the Up-stroke.

A. Velocity of the Down-stroke in very slow movements. Movement of the foot as slow as possible.

Subject.	Number Movements Measured.	Length of Down-stroke mm.	Time, σ.	Velocity, mm. per Second.	Metronome Tempo.
Sn.					c. 40 M.
		15	87	172	
		15	65	232.5	
		13	58	224	
					c. 20 M.
		34	48.5	702	
		30	93	410	
		35	68	516	Note that the stroke in the last three is longer, but time about the same.
Su.					c. 120 M.: as fast as possible (for comparison).
	13	28–16	64.6– 96.9	261–347	
Bg.					c. 25 M.
	6	32–34.5	144 –161.5	196–231	
Ha.					c. 20 M.
	4	30–31	64 – 64.6	465–478	
Ta.					c. 54 M.
	11	6.6–10	35.5– 61	145–853	Note that variation in Ta. records is due rather to length of stroke than to time of stroke.

B. Velocity of the down-stroke and up-stroke. Free movement of the hand and arm, as fast as possible.

Subject.	Number Movements Measured.	[1] Average Length Down-stroke.	Average Time of Down-stroke, σ.	[1] Average Rate Down-stroke.	[1] Average Length Up-stroke.	Average Time Up-stroke, σ.	[1] Average Rate Up-stroke.	Metronome Tempo.
Th.	1	81	113	742	81	158	516	c. 210 M.
	1	63	80.7	807	55	139	419.6	
	1	72	97	774	74	158	484	
	1	70	103	720	77	177.5	452	
	1	72	96.8	774	54	145	387.5	
	1	48	96.8	516	53	171	323	
	6	67.6	96.8	730	66	161.5	436	
	9	75	78.8	1033	74.3	184	426	c. 210 M.
St.	4	46	84	595	46	129	390	c. 240 M.
	4	51	80.7	646	49	126	390	
Bi.	14	42.6	48.8	952	43	138.7	336	c. 260 M.

[1] The lengths tabulated and therefore the rates are functions of the actual lengths. The actual stroke was about five times the recorded values.

In these very rapid movements the time of the up-stroke is much longer than the time of the down-stroke, and between the end of the up-stroke and the beginning of the down-stroke there is always a curve not here recorded, equal in duration to the down-stroke, so that *the total duration of the 'relaxation phase is at least three times the duration of the 'contraction phase.'*

may be regulated or changed at will. A third difference is noticed when two rhythms are beaten at the same time; the subordinate pulses of the one rhythm never occur during the beat-stroke of the other rhythm.

Nature of the Movement Beat-stroke. — However fast or slow the rhythmic movement, this high velocity is characteristic of the beat-stroke. In Table I., *A*, are given the duration values and the velocities of the beat-strokes in the slowest possible rhythms. It will be noted in the case of all four subjects that the duration of the beat-stroke is never more than 100 sig., and that the velocity is dependent on the length of the beat-stroke and not on the tempo of the rhythm. Under special limiting conditions the duration of the beat-stroke may be as long as 400 sig.,[1] but in all the records of freely-executed rhythms there are no beat strokes longer than 125 sig. and the velocity of the beat-stroke for the range of lengths which rhythmic movements present seems to be independent of the length of stroke. It is probable that if the movements were to be greatly exaggerated, differences of duration would appear; rapid movements of the eye show that the duration of the movement is affected by its length, though it does not vary directly. The duration of the beat-stroke is strikingly uniform, and is independent of either the tempo of the rhythm or the length of stroke. The clearest discussion of the events in such a rapid stroke brought to a sudden stop by the musculature, or by an obstacle, or by both, is found in Richer's discussion of human locomotion.[2] Richer divides all movements into three classes:

1. The two muscles or sets of muscles (*i. e.*, the antagonists, as the flexors and extensors, which produce the movement) contract simultaneously. (Ordinary type of slow movement.)

[1] Cf. Table II.

[2] *Traité de Physique Biologique*, D'Arsonval et Autres, Paris, '01. I., 'Locomotion Humaine,' p. 156 et suite. Richer, P., 'Note sur la contraction du muscle quadriceps dans l'acte de donner un coup de pied,' *Soc. de Biol.*, '95, Mars. 23. Athanasius, M. I., gives graphic records of similar movements obtained by a direct process, *C. R. de l'Acad.*, '02, 134, p. 311.

2. The one contracts, the other is passive. (Very excep tional type.)

3. The one contracts while the other relaxes and lengthens.

The rapid stroke in question is a form of this third type. This particular form of light, rapid, and sometimes repeated blow, Richer calls '*ballistic*.' By the study of instantaneous photographs he has determined that at the beginning of the stroke the positive muscle-set (extensors in case of the fore-arm) are suddenly contracted, but that by the end of the first third or first half of the movement, *the contraction of the positive muscle-set has ceased*, the positive muscles relax, and the limb is carried past this point by inertia alone. If the blow is delivered in the air, the negative muscle-set (in case of fore-arm, the flexors) contracts and stops the stroke. In rapidly repeated strokes of this kind, the member is returned to the original position by a similar sudden contraction of the negative muscle-set, which relaxes in the middle of the blow, and the stroke is in turn checked by the positive muscle-set. Thus the limb is *thrown* back and forth, and caught in turn at the limits of its movement by the positive muscle-sets. Such a battledore-and-shuttlecock type of movement is very aptly termed 'ballistic.' Rieger has recently described the same type of movement,[1] under the name of 'movements with elastic rebound,' but his form of record does not permit him to observe the action of antagonistic muscles as clearly as has Richer, and his concept of 'elastic rebound' is a better description than explanation of the movement. Rieger is aware however, that the beat-stroke of such a movement and its back-stroke make but a single unity in consciouness, and he promises to apply the principle of such a movement to the concept of accent in a future discussion.[2] As for the mechanism of innervation in such movement, Richer has no suggestion. Sherrington suggested[3] that 'reciprocal innervation' was the result of a fundamental structure of the muscle apparatus in antagonistic muscle-sets, but as Du Bois Reymond[4] points out there is no such fundamental grouping of

[1] Rieger, C., 'Über Muskelzustande,' *Zschr. f. Psy. u. Phys. d. S. org.*, '03, 32, S. 384 ff.

[2] *Loc. cit.*, S. 389.

[3] *Proceedings Royal. Soc.*, '97, p. 415.

[4] Du Bois Reymond, R., *Specielle Muskelphysiologie*, Berlin, '03, S. 243.

muscles into permanent antagonistic sets as the theory would require. Each set may be now antagonistic now synergic, and the rôle of muscles in the varied movements in which they take part is far too complex to admit of any fundamental neurological law of 'reciprocal innervation.' That such a reciprocal innervation does take place in the ballistic movement is evident, but it must be a type of coördination dependent on higher centers, and capable of all sorts of rearrangement, like other forms of complex muscular action and movement. What furnishes the precise signal for the action of the negative muscles in catching the limb tossed to them by the positive set? The case is different from that of an ordinary slow movement, where both sets of muscles are contracted, and the various inflowing kinæsthetic sensations check and guide the contractions of the muscles involved. The movement is very rapid; during the stroke the sensations are extremely vague, and as the experiments discussed later will show, frequently mislocated both in time and space. It does not seem possible that any event *after* the sudden contraction and relaxation of the positive muscle-set can be the cue for the contraction of the negative muscle-set. It is probable that the cue which sets the negative set to contracting is the *contraction of the positive muscle-set itself.* The brief interval between the positive contraction and the negative contraction, from 30 to 120 sig., is no more than time for a nervous impluse to be generated and reach the negative muscle-set.[1] The fact that time of the beat-stroke does not vary with the length of the beat-stroke, nor with the tempo of the rhythm favors this automatic connection of positive and negative muscles in the ballistic movement. It is experience alone which teaches us to guide the ballistic stroke. This experience is summed up in images of the movement (visual and 'motor'), which gauge the intensity of the positive muscle-set, and the appropriate contraction of the negative muscle-set follows inevitably.

An obstacle against which the limb strikes does not affect the character of the movement; at the end of the normal interval the negative muscle-set contracts and withdraws the limb, as

[1] Cf. Hofbauer, L., *Arch. f. d. ges. Physiol.* (*Pflüger's*), '97, 68, S. 553.

if the limb had shot to the end of its course unimpeded. It is simply as if the lower part of the oscillation had been cut off by the obstacle, and its place taken by a pause at the obstacle. If one closes the eyes and beats a rapid rhythm with the arm and hand, at first in the air, and then approaches an obstacle whose position is not exactly known until the hand strikes the obstacle

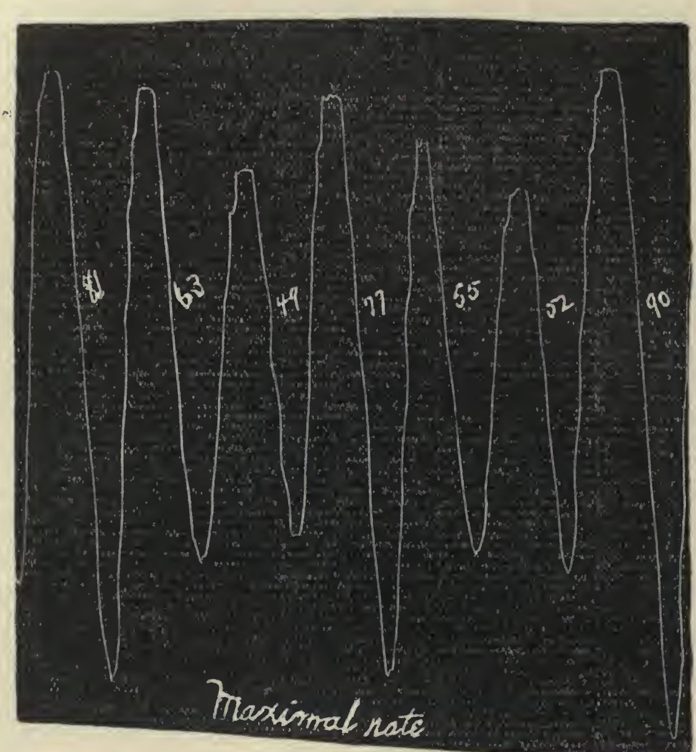


FIG. 1.

at each beat, one will find that the character of the movement and of the rhythm is quite unchanged by the intervention of the obstacle.

Nature of the Movement in the Back-stroke. — The relaxation-phase is not as invariable as the contraction-phase. In rapid beating the back-stroke begins immediately on the close of the beat-stroke; the record of such a rhythm beaten in the air shows a sharp angle between the beat- and back-stroke.

But, however rapid the rhythm, the velocity of the back-stroke is always much less (*cf.* Table I.), and at the end of the back-stroke, at the upper limit of the movement, there always appears a rounded curve which is very different from the sharp point at the lower limit.

In such rapid movement it is possible that the limb is driven back by a contraction of the negative muscle-set without any action of the positive-set. If so, the positive muscle-set must come gradually into action during the upper part of the back-stroke, for the rounded part of the curve at the upper limit of the most rapid movements can only mean that both sets are contracted, moving the limb slowly or holding it at rest just before the beat-stroke. During a slow rhythmic movement the limb often remains at the lower limit for some time, as if the negative contraction just balanced the momentum of the limb or the residual contraction of the positive muscle-set. In movements with great force at slow rates, it is certain that the positive-muscles are brought into play at the bottom of the beat-stroke, and coöperate with the negative muscle-set during the earlier part of the relaxation period in holding the limb. After this condition of rest at or near the lower limit of the movement for a longer or shorter interval, a very slow rise takes place (this may be modified in combined rhythms, as may be noted later) and the slow round of the curve at the upper limit shows that the two sets of muscles balance each other for a perceptible time just below the beat-stroke.

Although the phenomena of the relaxation-phase are thus somewhat variable, the relaxation-phase is a perfectly definite and essential process. The form of the rhythmic movement may be changed so that the limb does not return during the relaxation-phase to the upper limit for the next beat-stroke, but instead, the next beat-stroke starts from the point where the last beat-stroke ended. Measurements of records of such movements are given in Table II. The subject was directed to beat up and down with a baton, but to make a beat both on the down-stroke and on the up-stroke. The result is that every other beat-stroke occurs in the opposite direction from the inter-ʌening beat-stroke, and there is no back-stroke. What of the

process which the back-stroke represents in a rhythm-movement? On examining the records one sees that there is a long interval between the beat-strokes and during this interval the limb is at rest. (The pause is represented by a straight line on the moving kymograph cylinder.) (*Cf.* Fig. 2.)

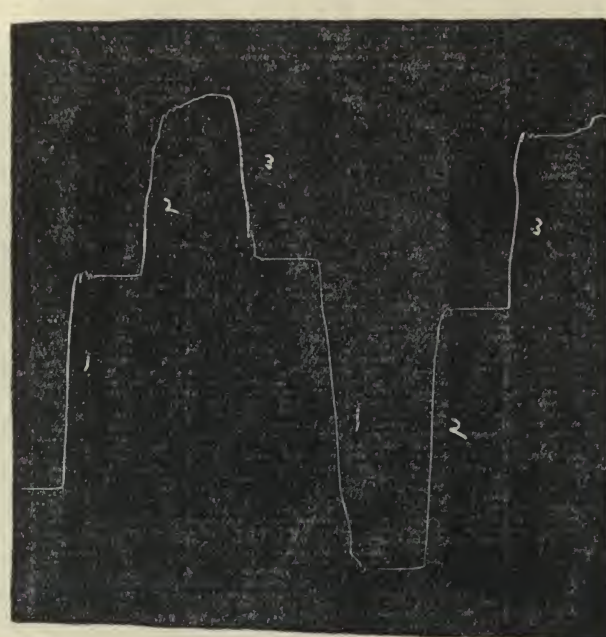


FIG. 2.

The duration of this pause appears from the measurements much longer than the beat-stroke, averaging from two to three times the duration of the beat-stroke. (*Cf.* Table II.)

The records of the three subjects were taken at medium and fairly rapid tempos, but the tempo does not affect the relative length of the pauses. After the experiments were performed on which Table II. is based, records of Rieger's were found which show the same relations at the fastest possible tempos. The records were taken for a different purpose, but the conditions are the same, and the movement was rhythmical.[1] At this maximal tempo, Rieger's figures show that at least

[1] *Loc. cit.*, S. 388.

Table II.

Comparison of the durations of the 'relaxation'- and 'contraction'-phases of a series of movements in which there is a beat both on down-stroke and up-stroke; these both become 'contraction'-phases and the pauses between them are the relaxation-phases.

Subject.	Number Movements Recorded.	Down-beat, σ.	Pause, σ.	Up-beat, σ.	Pause, σ.	Metronome Tempo.
Th.	1	355	387.5	193.5	452	70 M.
	1	355	387.5	226	355	
	1	355	484	193.5	387.5	
	1	323	484	226	484	
	1	291	452	226	387.5	
	1	387.5	613	161.5	419.5	
	1	355	419.5	226	484	
	1	226	419.5	291	323	
	1	387	452	226	452	
	1	355	355	226	484	
St.	12	Av. 317.5	Av. 312	Av. 196.5	Av. 201.6	82 M. Very little variation in values.
Bi.	12	297	490	193.6	323	76 M. Very little variation in values.
Th.	8	291	492.5	121	489	78 M. Very little variation in values.

Comparison of the durations of the 'relaxation'- and 'contraction'-phases of a series of movements in which there is a down-beat, then a second down-beat, then an up-beat, then a second up-beat, etc. (*cf.* cut 2).

Subject.	Number Movements Recorded.	Down-beat, σ.	Pause, σ.	Down-beat, σ.	Pause, σ.	Up-beat, σ.	Pause, σ.	Up-beat, σ.	Pause, σ.	Metronome Tempo.
St.										
	1	64.6	291	111.3	226	129	258	129	291	175 M.
	1	48.4	291	80.7	307	161.4	258	96.8	387.5	
	1	32.3	291	129	258	129	291	96.8	323	
	1	96.8	291	96.8	387.5	113	387.5	96.8	307	
	1	32.3	339	80.7	323	64.6	339	129	323	
	1	64.6	355	113	258	96.8	355	113	339	
	6	Av. 48.4	310	100	294	120	310	120	325	
Th.	6	80.6	310	122.7	184	100	310	84	339	140 M.
Bi.	6	113	403.7	132.4	397	119.5	452	122.7	384	120 M.
	6	190.6	336	119	355	132.4	387.6	193.8	355	155 M.

three fourths of the time was consumed in the relaxation-phase of the rhythm-movement. It is apparent that some preparatory process is necessary after one beat-stroke, before another beat-stroke can be made. Rieger's figures go to prove that this preparatory process takes more time when performed with the limb at a standstill, than when the limb is in motion as in the usual back-stroke.

This preparatory process must consist of a change in muscular tensions, since the limb is practically at rest. At the end of the beat-stroke, the negative muscle-set contracts sharply; in rapid beating at least, there is also a contraction at this point, or a residual tension, in the positive muscle-set. Except possibly in very slow beating, the contraction of the negative muscle-set cannot be so proportioned as to just balance the momentum of the limb, and tension in the positive muscle-set is necessary if the limb is to come to a standstill at the close of the beat-stroke. During the long pause which takes the place of the ordinary back-stroke the contraction of the negative muscle-set gives way to relaxation and the tension in the positive muscle-set becomes very slight. This is the poised condition just before the second beat-stroke.

The relaxation-phase alone is subject to control. Changes in tempo are due to voluntary hastening or retarding during the relaxation process. This fact is of considerable importance in conducting. A chorus or orchestra depend quite as much on the back-stroke as they do on the beat-stroke for direction. Conducting at the organ or piano is always unsatisfactory, and an angular style of beating which suppresses the back-stroke is almost as ineffective.

The events, then, in any rhythmic movement-cycle producing the simplest possible series of beats is as follows: At the beginning of the beat-stroke there is a sharp contraction of the positive muscle-set, setting the limb into rapid motion, during the earlier part or the first half of which the contraction of the positive muscle-set gives way to relaxation; this contraction-relaxation process in the positive muscle-set is the cue for the automatic contraction of the negative muscle-set; at the end of the flight of the limb, the negative muscle-set contracts sud-

denly and automatically and arrests the limb; this contraction of the negative muscle-set works against the momentum of the limb and a possible residual or active tension of the positive muscle-set; this ends the contraction-phase. After the contraction of the negative muscle-set has overcome the resistance at the bottom of the stroke, the contraction grows rapidly less, any tension between the two muscle-sets is reduced, and the condition of a very slight tension between the two muscle-sets, with the limb poised, is reinstated; this is the condition at the beginning of the beat-stroke (in the normal movement-cycle, the limb is raised at a varying velocity to its original position during the readjustment of the relaxation-phase) and the close of the relaxation-phase.

This movement-cycle is usually given its form and limits by a rather definite image of the movement. The upper limit with many subjects does not seem to be reached by a uniform movement through a certain time or during a certain definite change of tensions, for these subjects raise the limb at varying points in the cycle to the proper height and leave it at rest, awaiting the beat-stroke. But in itself this image of the movement would not give rise to any feeling of *movement*, or any impression of rhythm.

In the case of a rhythm beaten against a heavy resistance the figure previously published[1] is probably an approximate representation of the movement, but for the normal, freely beaten rhythm, the writer now considers the accompanying Fig. 5 a more accurate representation of the process (*cf.* Fig. 5).

Temporal Relations which Depend on Mere Accent. — Aside from the temporal relations of the typical unit-groups to be considered later, there are certain temporal relations which are to be referred directly to the nature of the movement-cycle of the single beat. The well-known lengthening of the accented beat, and the 'pause' which precedes the accented beat are due to modifications of the cycle. There are two ways in which the increased force of an accented beat may be obtained; either the length of the stroke may be increased (just as the upright-piano action is arranged to give degrees of loud and soft

[1] 'Rhythm and Rhyme,' *Harvard Psy. Stud.*, 1, p. 454.

by varying the length of the hammer-stroke), or the path of the limb may be unchanged and the contraction of the positive muscle-set more powerful. As greater length is usually associated with intenser contraction, probably both occur in untrammeled beating. Either of these ways will slightly modify the temporal relations of the resulting beats.

Although the time of the beat-stroke is little changed if the stroke is lengthened, the limb must be raised higher than usual, and this must add to the interval preceding. If there were any increase of the duration of the beat-stroke, that would be added to the preceding interval, for the ordinary methods of measurement make the end of the beat-stroke, or the limiting sensation the point of departure.

If the limb, in beating rhythmically, does not impinge on an obstacle, the accented beat will descend lower than the average beat, because its greater momentum will carry it farther. It will take more time for the relaxation process following the heavy beat, for higher strains must be reduced to the poised condition, and in untrammeled beating the limb must be raised through a longer back-stroke to the usual upper limit of the movement. As noted above (Fig. 1), an obstacle against which the limb strikes does not change the character of the movement; the action of the muscles is the same as if the limb had shot down to the end of the interrupted stroke. But it is customary to measure the beat from the point where the limb first touches the obstacle, so that a rhythm in which the accent is determined largely by increased contraction with the limb striking an obstacle will show by the ordinary method of measurement a decided increase in the length of the interval following the accented beat. The accented movement must always take more time however the increase is distributed before and after the beat by the particular method of measurement chosen.

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THE DIFFERENCE BETWEEN MEN AND WOMEN IN THE RECOGNITION OF COLOR AND THE PERCEPTION OF SOUND.

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(Communicated by Professor Howison.)

I. The Recognition of Color.

In testing the sensitivity of the eye to colors, Dr. Nichols found that men where more sensitive to red, yellow and green, while the women excelled in blue. He mixed a white powder with colored pigments. A series of mixtures, varying from white to clearly colored mixtures, were presented to the subjects (31 men and 23 women), who sorted them according to shade and hue. When once the women recognized the color of the compound, they were more accurate in detecting the degree of saturation of the various mixtures; yet with the exception of blue the women required more parts of the pigment mixed with the white before the tint of the compound was recognized.

Miss Thompson, on the other hand, finds that women are the more sensitive to colors. She tested 20 men and 20 woman with five colors, and found that the women could recognize squares of colored paper (red, green and blue) at a greater distance than could the men. The squares were pasted on cards, one black and one white. The tests were conducted in a dark room, the cards being illuminated by a Welsbach burner. Her conclusion is that men's eyes are surely less keen in the recog-

[1] Edward L. Nichols, 'On the Sensitiveness of the Eye to Colors of a Low Degree of Saturation,' *Am. Jour. of Science*, Series 3, Vol. 30 (1885), p. 37.

nition of the color of an object. "Yellow is the only color for which the men's record is better than the women's." [1]

By a different method than either of the two mentioned above, I attempted to discover the difference between men and women in the recognition of color. In the following experiments a Glan spectrophotometer was used; the source of light was a Welsbach burner placed .43 meters from the instrument.

Before beginning my work, August, 1903, I selected five bands of color in the spectrum, which appeared to me to be characteristic of red, yellow, green, blue and violet. The wave-lengths of the middle of the five bands were approximately, λ 6575, λ 5800, λ 5250, λ 4700 and λ 4450, respectively. The five bands were spacially equal in width, and measured about one twentieth of the length of the visible spectrum. During the tests all the spectrum was cut off except the band which stood for one of these five colors. The subject then looked into the eye-piece of the spectrophotometer, and was asked to observe and name the color, while the intensity of light was raised from 0° to a maximum, by slowly revolving the Nicol prism of the apparatus through an angle of 90°. The figures in tables I., II., and III., represent the readings in this angular scale, and not the absolute intensity of the light. The intensity of the light at any time varies approximately as the $\sin^2 a$; a being the reading on the scale.

Yet even at 0° there is some illumination of the spectrum (due to dispersion of light by the prism), so that at 0° every subject was able to see a band of light, which in general appeared white or gray. A few subjects could confidently recognize the color of the band at 0°. The violet end of the spectrum is of such low intensity that a blue-violet was chosen. The yellow strip selected includes all the yellow in the spectrum with a narrow band of yellow-green on one side, and a narrow band of orange on the other. A strip narrow enough to have included only pure yellow would have been too narrow to be used with the other colors, without being recognized by its lessened width, and, on the other hand, the width of the other colors could not

[1] Helen Bradford Thompson, *Psychological Norms in Men and Women*, University of Chicago Press (1903), p. 87.

well be reduced without making their area so small as to be recognized with difficulty by untrained observers.

Three separate hours were given to the experiment by each of the 40 subjects. During the first hour the subject was tested with Holmgren's worsteds for color-blindness. This took from 30 to 40 minutes. The room was then darkened, and the remainder of the hour was spent in practice upon observation of strips of the spectrum. The second hour the subject rested his eyes for 20 minutes in the darkened room before looking into the photometer. One determination was then made for each of the 5 colors, but with one eye only. The third hour was a repetition of the second, except that the other eye was used.

All conditions were kept as uniform as possible; the pressure of the gas was regulated by an automatic machanism and constantly noted; and the intensity of the color was increased at a constant rate of 1° per second. At the end of every 10 seconds, the subject looked away and rested his eye. Ten men and ten women used the right eye first, and an equal number of men and women used the left first. The order in which the colors were presented was varied; a given order was presented to but one man and one woman. The subjects were not told what colors would be shown them, nor that the same color would be seen for a second time. They were asked to look into the instrument, to report as soon as they saw any color, and to name the color as soon as they could recognize it.

There is a disadvantage in depending entirely on the name given to the color seen, but this was minimized as far as possible, for any peculiarity in naming the colors was noted when the subjects sorted the Holmgren worsteds; and when there was any doubt in my mind, or in the mind of the subject, the worsteds were brought out again and correction was made for any peculiarity in nomenclature. At times I could decide on no threshold (as in a case when the subject saw no green at all, but at the maximum intensity called that color yellow). It therefore happens that the average and median are at times calculated on less than 20 cases. I did not assume that the threshold for such subjects, as the one just mentioned, was greater than 90, for at 90 the strip was distinctly visible to him, and any increase in intensity

would only serve to make it more yellow. Even when green was suggested to him, he refused to accept it as a proper name for the color.

The results of these tests, according to one method of computation, appear in Table I. Using the average as the basis of our deductions, it would appear that the right eye of men is better than the right eye of women for all colors but violet; and

Table I.

Thresholds for the Recognition of Color (in Angles of Nicol's Prism).

Women.

	Right Eye.					Left Eye.				
	Red.	Yellow.	Green.	Blue.	Violet.	Red.	Yellow.	Green.	Blue.	Violet.
Average.. . .	11.6	13.8	11.3	25.0[2]	*24.3*[1]	8.8	*7.9*	8.0	28.8[2]	26.4
M. V. from Av.	8.4	9.5	5.1	11.2	8.0	3.7	5.6	2.5	15.3	9.7
Median . . .	*7.5*	10.0	10.0	20.0[2]	*20.0*	10.0	*7.0*	8.0	24.0[2]	25.5
Men.										
Average.. . .	*8.9*	*11.5*[2]	*8.0*[3]	*16.9*[2]	28.4[3]	*7.6*	13.7	*7.3*[3]	*17.4*[2]	26.6[2]
M. V. from Av.	3.6	6.7	2.0	7.3	11.5	5.1	8.3	2.4	10.8	10.4
Median . . .	8.0	*8.0*[2]	10.0[3]	*14.0*[2]	25.0[3]	*6.5*	10.0	*7.0*[3]	*14.5*[2]	*19.0*[2]

Table II.

Women.

	Right Eye.					Left Eye.				
	Red.	Yellow.	Green.	Blue.	Violet.	Red.	Yellow.	Green.	Blue.	Violet.
Average.. . .	8.4	*5.8*	9.9	17.5	*22.4*	7.5	*4.8*	*7.2*	18.8	25.1
M. V. from Av.	6.4	3.7	3.2	6.6	5.9	2.1	3.8	2.9	9.0	8.8
Median . . .	7.0	*4.5*	10.0	18.0	*20.0*	7.5	*4.0*	8.0	18.0	25.5
Men.										
Average.. . .	*7.7*	8.2	*9.4*[2]	*15.0*[2]	27.6[3]	*6.3*	7.0	12.0	*14.3*[2]	*23.3*[3]
M. V. from Av.	4.6	3.4	3.2	7.0	11.0	4.6	3.6	8.5	2.5	6.8
Median . . .	7.0	7.5	*9.0*[2]	*14.0*[2]	23.0[3]	*5.0*	9.0	*7.5*	*14.0*[2]	*20.5*[3]

[1] An italic numeral in tables I., II., and III., indicates that it is less than the corresponding numeral for the other sex.

[2] 19 subjects.

[3] 17 subjects.

TABLE III.

Women.

	Right Eye. Red.	Right Eye. Yellow.	Right Eye. Green.	Right Eye. Blue.	Right Eye. Violet.	Left Eye. Red.	Left Eye. Yellow.	Left Eye. Green.	Left Eye. Blue.	Left Eye. Violet.
Average....	*5.1*	*3.8*	*7.0*	12.0	17.0	5.9	*3.4*	*6.4*	14.6	21.1
M. V. from Av.	4.1	3.4	3.5	7.2	8.3	3.4	3.2	2.8	8.1	9.2
Median ...	*3.5*	*4.0*	*7.0*	13.0	19.0	5.0	3.0	*5.0*	14.0	23.0
Men.										
Average....	6.5	4.6	7.5	*10.4*	*14.7*	*4.9*	3.6	6.8	*9.5*	*13.8*
M. V. from Av.	5.0	3.7	3.7	3.9	9.1	4.8	3.3	3.3	4.8	8.7
Median ...	6.0	4.5	8.0	*10.0*	*17.0*	*2.5*	3.0	7.5	*10.0*	*17.5*

that the left eye of men is better than the left eye of women for red, green and blue; equal for violet, and worse for yellow. The norm would lead to the same conclusion, but if the median is used in place of the average, a somewhat different result is obtained. When we consider the large variation, we are probably not justified in saying more than that women are in general less keen in the recognition of colors. Yellow and violet are the only colors in which they seem to excel, and in these they excel with but one eye.

Yellow was the color most difficult to name at a low intensity, for the traces of green and orange on the edges of the strip selected confused the subjects. Only a few saw a uniform color. More men than women detected the presence of green and orange. The large threshold of both men and women is probably due to the inability to find an appropriate name for this complex, rather than to a low sensitivity to the color itself. The larger threshold of the men may possibly be due to their greater sensitivity to the green and orange, and consequently their greater confusion.

The women's failure to name the colors correctly at a low intensity cannot be due to their ignorance of the proper name, since at the maximum intensity they named the colors as accurately as did the men. Table IV. gives the different names applied to the colors as they appeared at the maximum intensity, with the number of men and women using each name. The parenthesis indicates that the subjects added those words after

being pressed for a closer description, and in many cases only after other colors had been suggested to them. In some cases the subjects gave two and even three names to the strip of color shown them. As the strips were practically uniform, with the one exception of yellow, it is probable that the knowledge that a

TABLE IV.

NAMES GIVEN TO THE STANDARD COLORS.

	Right Eye.		Left Eye.	
	Women.	Men.	Women.	Men.
Red.				
Red	14	12	17	11
Red (towards orange)	4	4		3
Red (towards purple)	2	2	1	2
Orange-red		1	2	4
Red-orange		1		
Yellow.				
Red, yellow and green	8	11	5	9
Orange-red and yellow	6	6	10	6
Red and orange	5		3	2
Orange		1	1	
Orange-yellow	1	1	1	3
Yellow		1		
Green.				
Green	8	5	3	3
Green (towards yellow)	3	4	6	3
Yellow-green	7	6	10	8
Yellow and green	1	2		2
Yellow, green and blue	1		1	
Orange and yellow-green				2
Yellow		2		
Green-yellow		1		2
Blue.				
Blue	7	8	6	7
Violet-blue		4	5	2
Green-blue	6	3	5	7
Violet, green and blue	6	3	3	1
Blue-green		1		1
Blue-violet	1	1	1	1
Blue and violet				1
Violet.				
Violet	12	14	14	5
Violet (towards blue)				3
Violet (towards red)				1
Blue-violet	6	2	6	8
Violet-blue	2	1		1
Blue		2		2
Blue (towards green)		1		

spectrum was being used helped to suggest the neighboring colors, though no doubt some subjects did actually discern a difference in the two edges of the band.

At the close of the tests, 10 men and an equal number of women were shown the whole spectrum, and asked to select the 5 portions which they called the best and purest bands of red, yellow, green, blue and violet. The results are given in Table V. The numbers represent the centers of the narrow

TABLE V.

READINGS IN CASE OF FREE SELECTION OF COLORS.

Women.

	Right Eye.					Left Eye.				
	Red.	Yellow.	Green.	Blue.	Violet.	Red.	Yellow.	Green.	Blue.	Violet.
Average	8.42	10.57	12.99	15.96	17.93	8.53	10.84	13.13	16.18	17.92
M. V. from Av .	.17	.19	.45	.27	.28	.18	.31	.34	.25	.24
Median	8.45	10.48	12.75	15.82	18.01	8.52	10.83	13.25	16.20	17.92
Men.										
Average	8.47	10.83	12.57	16.13	18.23	8.52	10.74	12.89	15.99	18.34
M. V. from Av .	.09	.21	.43	.60	.40	.14	.38	.41	.25	.55
Median	8.48	10.80	12.88	15.94	18.24	8.51	10.75	12.90	16.03	18.30
Readings actually used for standard colors	8.50	10.50	12.50	16.00	18.00					

band chosen, the visible spectrum running from about 7 to 22 — beyond 23 for some of the subjects. The standard colors used in the preceding tests are represented by the numbers 8.5, 10.5, 12.5, 16 and 18. The colors selected by the subjects are close to those used in the tests. The wave-lengths are approximately the same except for yellow and green. λ 5700 was chosen for yellow by the left eye of women, and by the right eye of men; and λ 5150 was chosen for green by the left eye of women. This is consistent with the fact that so large a number called the standard green a yellow-green (see Table IV.). From the names given to the standard violet we might expect the subjects to choose a violet farther away from the blue. It is true that the men did, yet the difference between 18 and 18.34 is not perceptible. In endeavoring to choose the colors on dif-

ferent days, I find in my own case that the variation from the average is between 1 per cent. and 1.5 per cent. for red, yellow, green and violet, and a little over 2 per cent. for blue, being but little less than the variation from the average of the 10 women.

Instead of waiting until the subject could name the color with certainty, we might take, as the more probable threshold of recognition, the reading where the color was first correctly named, even though it was named with doubt and reservation, and also the reading where the subjects gave a name so near the standard as to make it probable that they recognized the standard color, but had not secured the most appropriate name. For comparison I have calculated this probable threshold and in so doing I have allowed myself some freedom. I have not taken the lowest reading when my knowledge of the subject and the evidence of later reports made it certain that the first correct report was merely a lucky guess.

Comparing these results, given in Table II., with those given in Table I., we see that woman's record with the right eye is here lower than man's for yellow as well as for violet; her record with the left eye is still lower for yellow, and in addition is probably lower for green. These results, differing as they do from those obtained from Table I., may simply mean that the men were more cautious, that they did not venture a name until they were fairly sure it would fit; while the women would speak as soon as they discovered the region to which the color belonged, afterwards locating it more exactly.

Going over the original data for a third time, I calculated the thresholds in still another way. The threshold is now taken to be the reading where some closely kindred color or some component of the color, as it appeared to the subject at the maximum intensity, was named. For example, if at the maximum a color appeared green-blue to a certain subject, then we may assume that at the first mention of either blue or green, the color was really perceived by that subject. Table III. shows the thresholds calculated in this way. Orange was accepted as a correct answer for either red or yellow; if the green seemed a yellow-green, then either yellow or green was accepted for

green. Ample allowance is thus made for individual differences of nomenclature; and we have a threshold of recognition of something, which is a close approach to the standard, if it is not the standard itself. Woman's right eye is superior to man's right in this kind of recognition when the standards are red, yellow and green; her left is superior to man's left when the standards are yellow and green. The superiority of men in blue and violet is, on the whole, more marked than is the superiority of women in yellow and green.

When we compare Tables I., II. and III., we find that in spite of some discrepancies, there is a certain uniformity. In every instance the men have a lower threshold for blue. For the left eye they have a practically equal or lower threshold for violet; for the right eye a greater for violet, except when blue or violet-blue is accepted as a correct answer. It seems certain that the men could distinguish blue and the blue factor in the violet at a lower intensity than could the women. In the blue of the 3 tables there is at no time more than 4 men who fail to excel the average woman with the left eye; and never more than 7 men who fail to excel the right eye of the average woman. With the exception of the right eye in Table I., and possibly the left in Table III., women have the lower threshold for yellow; yet there are always from 6 to 10 women who fail to excel the average man. Women excelled in red when orange was accepted as a correct answer. The instances when they excelled in green are those in which yellow and green-yellow are accepted as correct answers.

Taking into account all 3 methods of calculating the thresholds, the general conclusion would be, that men are clearly superior in the recognition of blue; and women are possibly superior in the recognition of yellow. These results do not agree at all with those of Miss Thompson, who found that men excelled in yellow alone. The difference between my results and those of Miss Thompson may be due to the fact that 14 out of the 25 men she tested fell into the two classes, which she designates as 'color-blind' and 'poor in color discrimination'; while none of her women were 'color-blind,' and only 4 were 'poor.'[1] No color-blind person is included in my report,

[1] *Psychological Norms*, page 88.

although 5 men and 3 women might be called poor in color discrimination. Their mistakes consisted in mixing some of the blues and greens, and in failing to detect anything but a pale tint of red in our laboratory sample of Holmgren's purple. In naming the spectrum colors, however, these subjects appear to be as good as the average subject tested.

Miss Thompson finds that the left eye of woman excels her right in all but yellow. In my tables her left eye seems superior in yellow and green; yet when the number of individuals are counted up, there are but 11 whose left eye excels the right in yellow, and 14 in green; for the other three colors there is no difference between the two eyes. Miss Thompson finds man's left eye has a better record in yellow alone. I find that in yellow there are 12 men who have a better right eye, against 6 who have a better left; yellow is the only color in which his right eye excels. In red and green I find no difference, but in blue and violet the left eye excels; in blue 5 men have a better right eye, 8 a better left; in violet 9 have a better left, 3 a better right.

Each of the subjects was asked which eye he preferred to use. Most of them had no preference. Of the 20 women, 5 prefered to use the left eye. Each gave, independently, the same reason; in looking into the instrument with one eye they alway kept the other closed, and could with less effort keep the right eye closed. One, a left-handed woman, prefered to use her right for the same reason, that is, she found it easier to keep her left eye closed. Only 2 men expressed a preference; both preferred to use the right, they were in the habit of looking into instruments and were accustomed to using the right eye. In the results of these subjects I can find no consistent advantage of the preferred eye over the other.

II. The Perception of Sound.

The following experiments were undertaken with the object of finding any difference that may exist between men and women with regard to the absolute threshold for hearing. The individuals tested, 20 men and 20 women, were all students in the University of California.

An electric tuning-fork of 100 double vibrations was used in these tests. The noise which attends the sparking of the electric contact was avoided by connecting up the fork in series with a second fork of 100 vibrations in a distant room; this second fork interrupted the current in the first and permitted its use with continuous closed contact, and thus a noiseless pure tone was obtained. By means of a galvanometer and resistance the current was kept constant; and by the constant use of commutators any permanent alteration in the magnet of the tuning-fork was prevented. The subject sat in an ordinary research room, behind a screen in which was an aperture large enough to receive the ear. During the tests the subject sat with his head pressed against the screen, his ear within the aperture. The intensity of the sound was altered by varying the distance of the tuning-fork from the screen.

For each of the subjects, a first rough estimate of the greatest distance at which the sound could be heard was made. Then beginning somewhat within this distance, tests were made at intervals of 2.5 centimeters. At each interval a group of 10 tests were made, 5 with the tuning-fork going and 5 with it stopped. The order in which the two kinds of tests were taken was constantly varied. All the series of groups were run from above to below the threshold. The subject was given a warning; he then placed his ear at the aperture and reported whether he heard, or did not hear the sound. When the subject was doubtful the test was repeated. Between the groups the subject relaxed his position and rested. The greatest distance at which 80 per cent. of the answers were correct was recorded as the threshold, though groups of tests were also made beyond this point to make sure that the falling off of correct answers was not accidental nor momentary. The thresholds are recorded in Table I.; the figures represent the number of centimeters from the screen. The average and also the median of the 20 subjects are given. One man's threshold was more than twice as great as that of any other one of the 40 subjects. I have therefore also given the average of 19 men, excluding this one. Among the women there was none that seemed exceptional.

TABLE I.

THRESHOLD FOR HEARING IN CENTIMETERS. SERIES I.

	20 Women.		20 Men.	
	Right ear.	Left ear.	Right ear.	Left ear.
Average	44.75	38.00	70.25	53.50
Median	42.50	35.00	53.75	48.75
			19 Men.	
Average . . .			54.50	48.50

VARIATION FROM THE AVERAGE.

20 Women.		20 Men.	
Right ear.	Left ear.	Right ear.	Left ear.
9.8	9.1	21.0	12.8
		19 Men.	
		8.7	7.7

The average women of the 20 could hear 17 per cent. farther with the right ear than with the left. The average man of the 20 could hear 31 per cent. farther with the right than with his left. The average man of the 19 could hear 12 per cent. farther with his right than with his left.

The average man of the 20 could hear 19 per cent. farther with his left ear than the average woman could hear with her right ear. The average man of the 19 could hear 8 per cent. farther with the left ear than the average of the 20 women could hear with her right.

The results show that both the women and the men could hear farther with the right ear than with the left. The men hear much better than the women. There was but one woman who excelled the average man; and, on the other hand, only 3 men fall below the average women. Eight women fall below the lowest man. The men not only could hear further than the women, but the poorer ear of the men was much keener than the better ear of the women.

A second set of experiments was then undertaken, this time with a tuning-fork of 500 double vibrations. The same subjects, with the exception of 2 men and 2 women were used in these tests. More precautions against external noises were taken. The subject sat in a silent room, from which all noises were excluded by specially constructed walls and doors. One end of a lead pipe has its termination in this silent room, while the

other end terminates in a research room in another wing of the laboratory. The screen used in the first series of tests was set up in the silent room, 1.675 meters from the end of the lead pipe. The tuning-fork of 500 vibrations was set at varying distances from the other end of the pipe in the distant room where the experimenter was. The appearance of a light in the dark room served to warn the subject, who then, as before, placed his ear at the aperture in the screen. Here and in the earlier series of experiments, half of the men and half of the women used the right ear first, the other half used the left first. The subject communicated his answers by means of a telegraphic key. After a first rough estimate of the threshold was made, 10 tests were made at regular intervals of 1 centimeter. The method used was the same as the first group of tests. The figures in Table II. are in centimeters, and represent the distance of the tuning-fork from the end of the tube.

TABLE II.

THRESHOLD FOR HEARING IN CENTIMETERS. SERIES II.

	18 Women.		18 Men.	
	Right Ear.	Left Ear.	Right Ear.	Left Ear.
Average.	6.88	5.66	8.61	7.77
Median	7.00	5.50	8.50	6.00

VARIATION FROM THE AVERAGE.

20 Women.		20 Men.	
Right Ear.	Left Ear.	Right Ear.	Left Ear.
3.50	3.16	5.00	4.80

The average woman could hear 21 per cent. farther with her right ear than with her left ear. The average man could hear 10 per cent. farther with his right than with his left ear.

The average man could hear 11 per cent. farther with his left ear than the average woman could hear with her right ear.

The results are as before: the men hear farther than the women; the right ear of both men and women is keener than the left. The changed conditions and the difference in the note has not changed the general result, though it affected the relative positions of individuals within the groups of subjects. The two men, whose records were the highest in the tests with the tuning-fork of 100 vibrations, fell below the average of the men

when the fork of 500 vibrations was used. The two women who stood highest in the first series of tests also stood highest in the second series; but in general it is not true that the women who heard farthest in the first series, also heard farthest in the second series. Fully 50 per cent. of the men whose records were higher than the median in the first series, fell below the median in the second series; the same is true of the women, although the women do not make such a decided change. They change from a short distance on one side of the median to a short distance on the other. The superiority of the men is not so marked. With the left ear, 7 women excel the average man; with the right ear, 4 women excel the average man. Eight men fall below the average woman with both the right and left ear.

The superiority of the right ear over the left is as marked in this second series as it was in the first. Not only was the right ear of the average man and average woman better than the left, but the right ear of almost every individual subject was either better than, or equal to, the left (see Table III.). Of the 40

SERIES I.

Number of women with a better right ear	16
Number of men with a better right ear	14
Number of women with a better left ear	1
Number of men with a better left ear	2
Number of women with the right and left ear equal	3
Number of men with the right and left ear equal	4

SERIES II.

Number of women with a better right ear	12
Number of men with a better right ear	14
Number of women with a better left ear	3
Number of men with a better left ear	3
Number of women with the right and left ear equal	3
Number of men with the right and left ear equal	1

subjects, but one man was found whose left ear was better than his right in both series. Of the remaining 5 subjects whose left proved better than the right in second series, the right and left of two had been equal, the right of 3 had been slightly better than the left in the first series.

None of the 40 subjects knew of any defect in their hearing. They were asked if they knew of any difference between the

right and the left ear. Seven (2 woman and 5 men) thought the right was better; 5 (3 women and 2 men) thought the left better. None of these gave, or pretended to give, any good reason for their opinion; and in every case their opinion, when they thought better of the left, was not in accord with the results of the tests. The one man whose left ear proved to be better, knew of no difference.

The results indicating more acute hearing in the men cannot be due to a greater recklessness in answering. If this were true, we would expect the men to make more errors than the women when the fork was silent. On the whole they made fewer such errors. Many subjects made no errors of this kind. In the first series, 9 men made a total number of 46 errors, 20 with the left ear and 26 with the right; 9 women made 42 errors, 9 with the left and 33 with the right. In the second series the men were more cautious; 6 men made a total of 21 errors, 9 with the right, 12 with the left; 13 women made 68 errors, 29 with the right, and 39 with the left.

Many in this second series did not use the signal 'doubtful' at all. The number of such answers is given in Table IV. In this second series the left ear of both men and women was not only less acute, but more doubt was expressed and more errors made when it was used; the variation from the average is also slightly greater.

TABLE IV.

SERIES II.

Right ear, fork sounding.

45 'doubtful' answers were made by 14 women.
95 " " " " " 16 men.

Right ear, fork silent.

6 'doubtful' answers were made by 4 women.
2 " " " " " 2 men.

Left ear, fork sounding.

53 'doubtful' answers were made by 13 women.
116 " " " " " 16 men.

Left ear, fork silent.

No 'doubtful' answers made.

Fechner[1] found the left ear to be better than the right. His

[1] In *Poggendorf's Annalen der Physik und chemie*, vierte Reihe, Band 111. S. 500.

method was to place a watch directly before the subject, who after closing with the forefinger first one ear then the other, stated in which ear the watch seemed louder. Such a method records only the subject's opinion. I found that results obtained by a test similar to Fechner's were not in accord with the results obtained by the tests with the tuning-fork. The subjects were the same 18 men and 18 women used in the second series of tests with the tuning-fork. A watch was held in front of the subject, who, after turning the head slowly from side to side, stated in which ear the watch seemed louder. Eight (4 men and 4 women) said the watch seemed louder in the left ear. The former tests seemed to show that the left ear of but one man was better; the right ear of the remaining 7 had been shown to be better. Five (1 man and 4 women) said the watch seemed louder in the right; the remainder of the subjects could tell of no difference. Again, I brought the watch from a point beyond to a point within the range of hearing, and asked the subjects with which ear they first heard the sound. The 8 subjects mentioned in the first test with the watch, answered that they heard it first with the left ear; and the same 5, who before seemed to hear it louder with the right, now said they heard it first with the right; the remainder of the subjects could tell no difference. There thus seems to be a slight tendency to think better of the left ear, even when that ear is the poorer. It is of course possible that if a tuning-fork had been used instead of a watch a different result would have been obtained.[2]

[2] The MS. of this article was received March 11, 1905.—ED.

EXTENSITY AND PITCH.

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Psychologists in general agree that 'sensation' has the four characters of quality, intensity, duration (or protensity), and feeling-tone. To these some would add volume (or extensity), and others would add vividness. There seems to be no reason why the list should not be somewhat further extended by adding local significance, which may with advantage be distinguished from quality; and meaning, or symbolic value, which is at least as important as any of the other characters. Even with these additions there is question whether the list is complete, but it is sufficiently so for present purposes.

These characters of simple sense objects are not parallel in their functions, but differ in their relation to the existence of these sense objects, and in their modes of variation. The first three which we have named seem to be essential; we can not conceive of the existence of a 'sensation' which has no duration, or which has no intensity, or which has no quality; *i. e.*, which is no particular kind of 'sensation.' Whether the same holds true for extensity is perhaps less certain, but there is strong evidence that way, as we shall see later. Vividness, feeling tone, local significance, and meaning, seem to be less essential. There conceivably may be sense objects present to consciousness which yet are of zero vividness; that is, they are either in the realm of so-called subconsciousness, or else they are neglected in their immediate nature, and their meaning alone taken into account. Local significance, again, may possibly be lacking; olfactory objects, in particular, never seem to have in themselves any local signs, although of course they are localized by the help of tactual and muscular experiences; something of the same kind seems to be true of auditory sensations also. It may be that local signs exist only in touch and sight;

or on the other hand it may be that there is a system of local signs present in hearing which is represented by the system of pitches; but at least we can conceive of 'sensations' which should be entirely devoid of this character. Suppose, for instance, two nerve endings which should when stimulated produce sensual processes corresponding to the same sensuous quality; there might be in these processes nothing which would enable us to distingnish one from the other, and yet both would be real.

Sense objects may be neutral in feeling-tone, which is equivalent to their having no feeling tone. Some psychologists insist on calling neutrality a definite feeling-tone, thereby making feeling-tone an essential character; but this seems an uncalled for complication, since feeling-tone signifies the character by which states of consciousness differ as regards pleasantness (or unpleasantness, whichever you chose) and when this factor is reduced to zero there is no better excuse for saying that it still exists than there is for saying that zero intensity and zero duration are still existent intensity and duration.

Meaning, likewise, is not necessarily present. A sense object may conceivably be taken for just what it is, without reference to anything else. Of course, the more experience we have, the more nearly impossible this becomes; but even so we may hold that the absence of meaning does not imply the nonexistence of 'sensation.'

These various characters of simple sense objects show also diverse types of behavior in the analysis of psychical compounds. The mere quality of a compound involves nothing which is not in the qualities of its components. So the intensity and duration are direct functions of the intensities and durations of its elements. Or, if the more accurate form of statement is preferred, we may say that the qualities, intensities, and durations, of the elements into which the complex state is analyzed will include all qualities, intensities, and durations found in the complex, and no other; there is neither surplusage nor deficit resulting from the analysis. The complex as a whole has no quality which cannot be reduced to the qualities of the elements; no intensity which cannot be reduced to a summation of elementary intensities; and no duration which is different from the duration of a definite number of its elements.

Extensity falls in with duration. But in both these characters we must be prepared for an apparent surplusage resulting from analysis, which apparent surplusage is due to the phenomena of superposition; *i. e.*, simultaneity in time or collocation in space. When we take these factors into account the apparent post-analytical redundancy disappears.

With regard to the other characters of elementary sense objects, this simple analytic relation does not hold. There are factors in the feeling-tone of a complex that are not assignable to any of the sensuous elements into which the complex may be resolved, but which seem to belong to the complex as such, or perhaps to intellectual elements. The meaning of a complex is far different from the total meanings of its elements. Vividness is scarce amenable to analysis at all, for to a certain extent the vividness of the complex as a whole is inversely proportional to the vividness of its elements. Local significance may become entirely lost in a complex, especially when the complex involves the coöperation of two or more senses, and it appears in these cases only by the analysis of the complex into its elements.

With regard to their analytic behaviour therefore, as well as their essentiality, the feeling-tone, meaning, vividness, and local significance characters differ radically from quality, intensity, duration, and extensity.

There is however one way in which quality differs fundamentally from intensity, duration, and extensity, and that is in its method of variation. The variations in these last three are continuous from zero to the highest possible value, without any points of special value, *i. e.*, determining points, in the continuum; while the variations of quality pass through definite special or determining points with transition regions between. This difference is formulated in another way in the statement that variation in quality is variation in kind, while variations in intensity, duration, and extensity are not in themselves variations in kind.

As regards the physiological concomitants of at least five of these characters of simple sense objects, we can speak with confidence. Quality is essentially correlated with the kind

of end organ stimulated (or perhaps with the kind of process which is aroused in the end organ and brain cell). Intensity is correlated with the intensity of the process aroused. Duration is identical with the duration of the process, presumably in the central cell. Local significance is the correlate of the particular end organs stimulated; we may suppose each end organ to have its particular local sign. Extensity has in this respect its full title proven, since it is obviously the correspondent of the number of contiguous end organs stimulated.

The physiological conditions of the other characters — feeling-tone, meaning and vividness — are much more complex, and hence, though we assume that they are perfectly definite, are not readily assignable.

This scheme, of four *essential* characters, and four which may be called *accidental*, is satisfactory until we come to the consideration of sound sensations, at which point it seems at first unsatisfactory. Have we any character with which *pitch* can be identified? Duration, intensity, vividness, feeling-tone and meaning are of course excluded from the possibility, since they all apply to sounds over and above pitch; so quality, extensity and local significance are left to be considered. Quality, however, is soon put out of the running, since we have shown that in the other senses it varies through various determining points which are few in number for each sense; whereas pitch varies in a continuum without internal orientation, exactly as do intensity, duration, extensity and vividness. Local significance is the character which we should urge in this connection, if we held to the Helmholtz theory of audition, but since the present disposition is to look upon that theory as mechanically untenable, we should be obliged to look for further reasons for our choice, and such reasons, apart from the necessity of finding some character with which the identification may possibly be made, are not forthcoming. On the contrary, it seems impossible that pitch can rest upon local signs, since local signs do not in general vary between two extremes, but rather include a manifold of differences which do not admit of easy schematization. The same fact is expressed when we say that intensity, duration, extensity, and likewise pitch, admit of quantitative comparison, while local signs do not.

Extensity, however, furnishes conditions which correspond in every particular to the properties of pitch. It varies continuously between extremes, admits of quantitative estimation more or less exactly, and moreover is directly connected with pitch in introspective analysis. The so-called 'high' notes are *small*. The 'low' notes are *large* or *voluminous*. Differences in pitch, in other words, are directly comparable to differences in planar or linear extent, and the physiological condition of difference in pitch accordingly is probably difference in number of nerve-endings stimulated.[1]

This theory of pitch is much more in accordance with the known facts of tone perception than is the Helmholtz theory. Let us consider first the discrimination of overtones. According to the local sign theory there ought to be very little difficulty in recognizing the octave when sounded with the fundamental, for the two components are dependent on the stimulation of two nerve endings or groups of nerve endings which must be relatively a considerable distance apart in the series and hence as easily discriminable as two points of light on the retina. On the extensity theory, however, the nerve endings which the higher note stimulates are all stimulated by the lower note; that is, the higher note is contained in the lower note both psychologically and physiologically, just as if a short streak of light were superposed on a long one; so that the discrimination where the notes harmonize (*i. e.*, where there are no beats), and where the lower (or larger) note is not much less intense than the higher (or smaller), should be rather difficult, which is actually the case.

Moreover, if differences in pitch depended on differences in individual nerve endings stimulated, there would be no reason for expecting the lower note in a complex to dominate, *i. e.*, to give its pitch to the complex. But if the difference in pitch is really a difference in volume, we might well expect the larger to determine the size of the total complex, as really is the case when the lower note is not too weak.

[1] Subsequent to the construction of this theory on purely psychological grounds, I found that Ter Kuile had constructed a physiological theory with which it practically agrees. See *Pflüger's Archiv*, 1900, Vol. 79, pp. 146–157 and 484–509.

In the third place, as regards a changing pitch, we have on either theory something analogous to a perception of motion; but on the local sign theory the analogous motion would be that of a point generating a line in the field of vision or of touch; while on the extensity theory the proper analogy would be a line increasing or decreasing in length; *i. e.*, there is a certain part of the object which remains unchanged by the variation. This is a point on which introspection may differ in different cases, and on which it is hardly trustworthy on account of the sophistication of our auditory sense; in my own observation, however, the shrinking or expanding in linear extension in the field of vision or touch is a perfect analogue of the shifting pitch, and the moving point is not at all applicable.

Finally, the peculiarities of the complex sound we call 'noise, fit in perfectly with the proposed theory. The analytic characteristics of a 'noise' are: (1) excessive complexity, which is the essential feature, and may be the only one except for the beats to which it gives rise; (2) progressive variation in the intensity and pitch of the components, which is an accidental feature, but which adds greatly to the 'noisiness'; and (3) indefiniteness in pitch of the complex determined by the first two characteristics.

The reason for this indefinitness of pitch becomes clear as soon as we consider the analogy to light sensations. Suppose we superpose many streaks of light of various lengths, making one end of each streak coincident with the corresponding ends of all the others. The result will be a streak of light relatively very intense at one end and fading off at the other, so that the length of the total illuminated area is indefinite. This is just what happens in the case of a noise; the superposition of the variety of tones makes the exact limits of the complex difficult of determination, although there is a *general* pitch distinguishable.

The designation of pitch as the form which extensity takes in auditory sensation seems to be the most satisfactory clearing up of the field of sensation characters. Not only does it dispose of the vexed question of pitch, but it also helps to confirm the right of extensity to be considered an essential character of sensation, instead of an accidental character.

[1] The MS. of this article was received February 2, '05.—Ed.

A MOTOR THEORY OF RHYTHM AND DISCRETE SUCCESSION. II.

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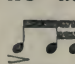
2. *Nature of the Unit-Group.*

Since accentuation and subordination are contributed in rhythmizing a perfectly uniform objective series, they have always been considered important, and perhaps essential elements in rhythmic perception. The most fundamental type of subordination with accentuation is to be found in the unit-group.

Many different types of feet in verse, of measures and figures in music, and of 'groups' in psychology have been named and described. The musical measure is manifestly a secondary and not a primary type of grouping, and the 'groups' of the psychologist have usually been arbitrary logical constructions, so that a classification of unit-groups need include only poetic feet and musical figures. Despite the many combinations named, most recent writers are inclined to reduce the types of feet to four: iambic, trochaic, dactylic and anapestic.[1] Some writers add amphibrachic, but it is only a variety of the dactylic. All the numerous forms of feet occasionally given can be separated into these elementary forms. A consideration of musical figures shows that these forms of poetic feet cover many of the varieties of musical figures. The trochee represents such forms as , , , and the more unusual . The iamb represents the 'dotted-eighth-and-sixteenth forms, . (usually written ), most grace notes, and the unusual  and . The dactyl represents the triplet  and the unusual figure  (in most cases this notation

[1] Cf. Gummere's Poetics.

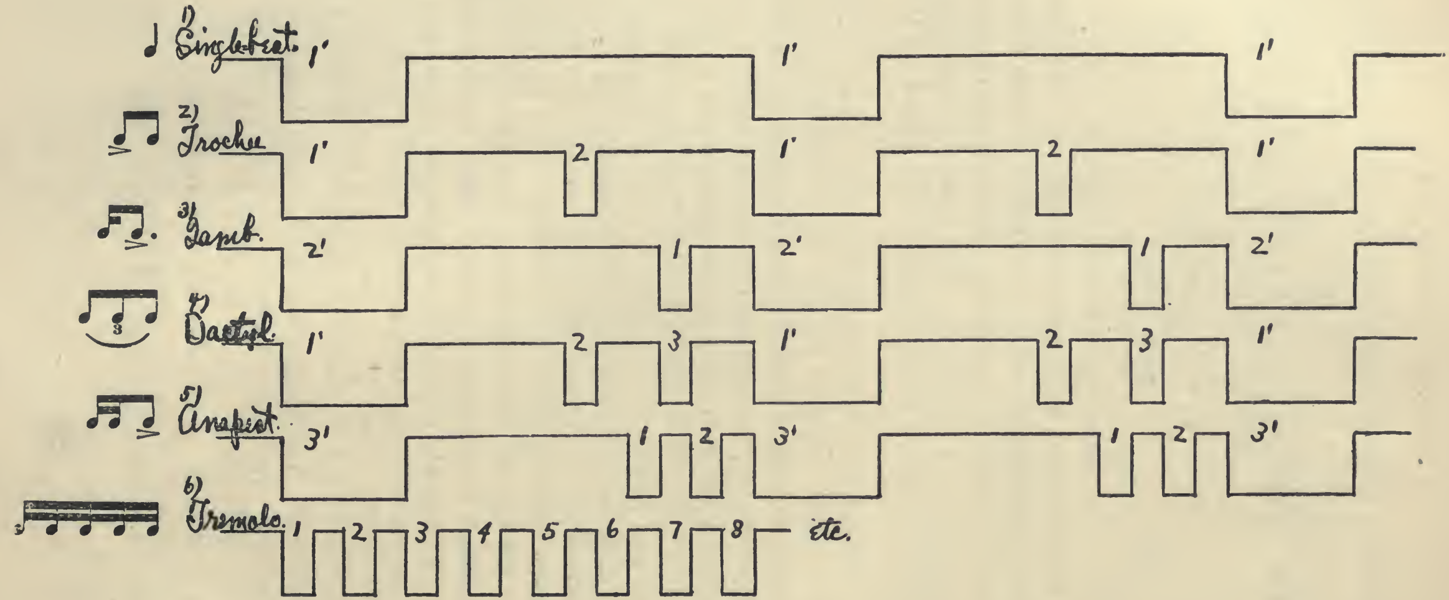

1) Singlebeat. 1' 1' 1'
2) Trochee 1' 2 1' 2 1'
3) Iamb. 2' 1 2' 1 2'
4) Dactyl. 1' 2 3 1' 2 3 1'
5) Anapest. 3' 1 2 3' 1 2 3'
6) Tremolo. 1 2 3 4 5 6 7 8 etc.

FIG. 3.

indicates not a unit-group but a compound of ♪ and ♬). The reverse of this last type ♬ 𝅘𝅥 is represented by the anapest, but it is worth noting that in music where this form occurs as a true unit-group (the notation usually represents a compound) it becomes simply a variety of the iambic ♪ 𝅘𝅥. There remain two unit-groups in music not represented by the poetic feet mentioned; these are the single note 𝅘𝅥, 𝅗𝅥, etc., the trill or tremolo, and the rapid run. The trill or tremolo and the rapid run all represent one type of figure ♬♬♬ in which the number of constituent notes is indefinite, and the intervals between the constituents are all equal.

Below is given the complete list of unit-groups which must be considered; the time relations are represented and the width of the depressions indicates the length of time that a finger will remain on a key, or a syllable will be vocalized; the groups are arranged so that the accented beats, the convenient reference points, coincide.

The length of time that the finger remains on the key is really a function of the energy of accent; the untrammeled movement would give the form in Fig. 4. (*Cf.* Fig. 1.)

Fig. 4.

There is an upper and a lower limit to the rate of succession at which grouping into unit-groups is possible. The lower limit is variously estimated at 1.5–3 sec., 40 M.–20 M.. As this interval of 1.5–3 sec. represents the longest interval during which a rhythmic movement can persist, it is obvious that a group of twice that length (the simplest possible two-group) would be an impossible rhythm; therefore, at the lowest rate of rhythmic succession no grouping is possible, *i. e.*, the group must consist of one beat. When the rate of succession is increased to 4–8 per sec., the interval and the accent again become uniform and there is no grouping possible. The single

beat, then, and the tremolo are forms of the unit-group which occur at the upper and lower limits of rhythmic succession, and they are evidently not produced by the grouping activity as it occurs in the case of the iamb, trochee, dactyl (and anapest).

The notion that the muscular tensions of a movement-cycle such as that previously described must be responsible for the grouping of the unit-group [1] is too general as it stands. Miner [2] suggests that the fusing of reactions to stimuli in a muscle contracting and relaxing at certain rates is the cause of the grouping. Such a fusion of contractions is observed in the action of a single muscle, wheras the simplest rhythmic beat is produced by a complicated movement-cycle; the fusion would in no wise explain the characteristic structure of the unit groups. Moreover, if the rhythm is due to the *movement*, such a fusion might explain the *omission* of the subordinate beats, but not their grouping.

The Wundtian explanation that the beats of a group occur during a wave of attention, if translated into terms would mean that a secondary rhythmic movement occurs, and the beats of the primary movement included in each simple movement-cycle of the secondary rhythm are thereby grouped together. Such a combination of rhythms must actually occur in music, but it will not explain the unit-groups. First, in music where it does occur we do not have the temporal and accentual relations of the unit-groups;[3] second, with such a grouping mechanism, we should expect an indefinite number of beats in the unit-group (in music we do actually have an indefinite number of beats of one rhythm occurring during a single beat of the secondary rhythm); there would be no reason why the type of grouping at the fastest rate of succession should change, as it does when the tremolo form of rhythm appears.

Accentuation and subordination with the involved temporal relations are not peculiar to rhythmic movement. Wherever complicated movements occur we have such a grouping. Individual letters of words written with a pen, syllables and words

[1] 'Rhythm and Rhymes,' *loc. cit.*, p. 455 ff.

[2] Miner, J. B., 'Motor, Visual and Applied Rhythm,' Psy. Rev., '03, Mon. Sup. 5, No. 4, p. 34.

[3] Cf. 'Combined Rhythms,' below.

written at the typewriter, but best of all spoken words, illustrate this grouping of a set of movements about a single definite accent. As one writes the word 'the' at the typewriter, the form is ⏑ –́ ⏑ with a definite accent on the middle letter. In the syllable 'ing,' the form is ⏑ ⏑ –́ with the accent on the last letter. In writing the letter 'a,' the movements are subordinated to the down stroke at the side of the loop. But speech best illustrates such groupings. Every English word of more than one syllable constitutes a whole; it is in a sense a single movement, and yet it contains distinct pulses, and one of these pulses is accented and is much longer than the rest and is, in a sense, the center of the word. The uttering of the word is a muscular process and the varying lengths of the syllables are due to the accentual form. The accent itself is made by an increased contraction of the muscles of expiration; in fact, the whole expiratory stress seems to center in the accent. In the case of the movements in writing with the pen, observation shows that the subordinate strokes are performed by the fingers, while a stroke which has the accent is produced by the hand, or the hand and arm as well as the fingers. These strokes of the fingers, and of the hand and arm are all parts of the same movement; the muscles act together, at points all the muscles are working together, but at other points, the more mobile sets execute semi-independent movements while the slower sets are preparing for the next main contraction at the principal part of the movement. Most of our movements show this semi-independent, semi-synergic character of the various parts and muscle-sets of the organ involved. In the case of the vocal organs, the more mobile parts are the organs of mouth and throat, the slower are the muscles of expiration. In wielding any instrument or tool, the hands and fingers are performing a variety of auxiliary movements during the slower moving of the fore-arm; the upper-arm and shoulder are less used than the fore-arm for rapid parts of the movement. In walking or executing the pedale part of an organ score, the foot and leg show the same system of main and auxiliary movements as the hand and arm. In such cases we have a mechanism of subordination and accentuation which was developed long before

rhythmic movement made demands on it. It is important to note in all the cases mentioned of auxiliary movements that they are in no sense separated from the process of the whole movement; not only are they means to the one end of the general movement, but it is impossible to say where they begin or where they end, and at many points, the muscles are all contracting or relaxing in one common wave of innervation.

Rhythm is produced by a series of single strokes composing a movement of the type just noted; the fingers and hand, the hand and arm, the vocal organs, can all execute rhythmical movements. Granted that one of the beats be accented, and of necessity other muscle-sets will come into play with the accent, partly because of the tendency of innervation to irradiate to related muscles, partly because the increased strain will necessitate further bracing contractions (the 'pseudo-antagonistic' coordination).[1] These added muscle-sets will work as a part of the movement, and yet their rate of easy contraction and relaxation is much less than that of the more mobile muscle-sets involved. Even the different fingers of the hand show different degrees of mobility.[2]

While these muscles which have to do with reinforcing the accented beats are working slowly, the more mobile muscle-sets execute the intervening beats with less force and with less amplitude of movement; this beating of the unaccented elements is not a separate process, however; the minor muscle-sets unite with the major muscle-sets to make the accented beat, and many of the same muscles which are active during the accented beat must take part during the unaccented beats (*e. g.*, the muscles which brace and guide the limb as a whole). (*Cf.* Fig. 5.) The process remains one movement, just as the letter in writing, the word in speaking, the complex movements in striking a chord at the piano or in turning a door-knob are all one movement. But why should the accentuation develop in a series? If the rate of a very slow series be increased, or if the movement of any series be intensified, other muscle-sets will be inevitably

[1] *Cf.* Beevor, C. E., 'Muscular Mechanism,' *Lancet* (London), '03, 1, 1357–1360; also Müller, R., 'The Mosso Ergograph,' *Phil. Stud.*, 17, S. 1.

[2] Reif, O., 'Ueber Fingerfertigkeit beim Klavierspiel,' *Ztschr. f. Psy. u. Phys. d. S. org.*, '00, 24, S. 352.

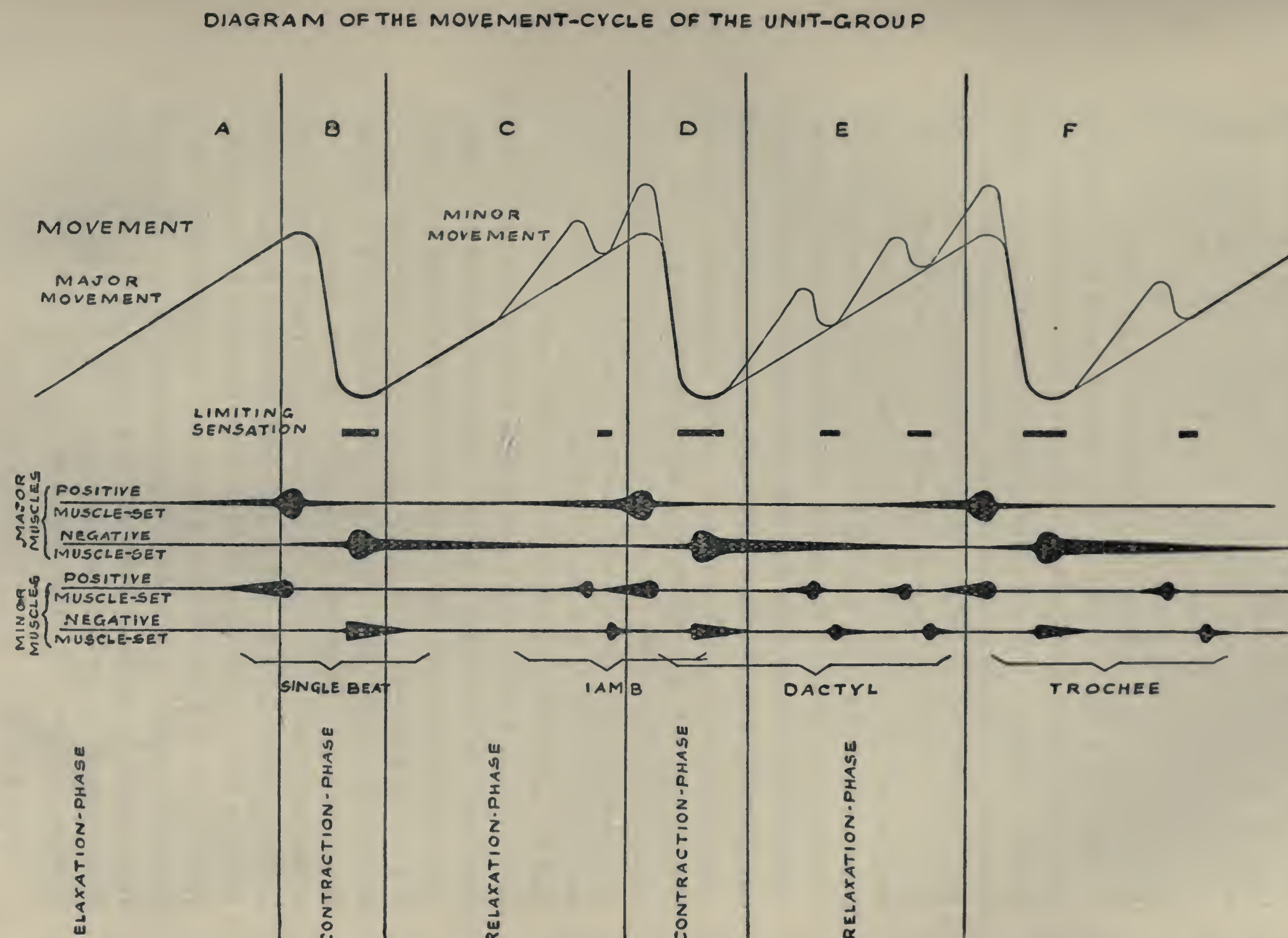


FIG. 5. Contraction of the muscles is represented by the increase in width, relaxation of the muscles by the decrease in width of the line representing the muscle-sets.

involved, and as these work at different rates, the accentual type of coördination previously developed by all sorts of movements is the one natural and easy way for them to work together. One may say that the unit-group is the form in which the various muscle-sets and segments of a limb or organ can all work together freely and easily in a single-movement cycle.

What are the conditions in which the parts of a complex movement seem *continuous*, and the movement therefore a whole? Rieger[1] notes that in ballistic movements the back-stroke is psychologically continuous with the beat-stroke and with it makes up a unity. When a new movement is learned its parts are executed separately and are distinct; the muscular tensions die out, and each is a new movement. Even when the movement comes to be executed more rapidly, as long as there is hesitation between the parts, the movement does not seem continuous, even though the muscular tensions are maintained during each slight pause. When the movement finally becomes continuous, the muscle-sets are acting reciprocally and the muscle-tensions are constantly changing; the parts of the movement 'flow into each other.' As long as the tensions are changing between the muscle-sets we have continuity. When the tension between the muscle-sets is stable, the limb is poised and the movement is at a standstill and there is an impression of suspense, of delay, or of 'pause,' but not of continuity. The degree of tension in this condition of poise and standstill determines the intensity of the feeling of suspense and delay. When the tensions between the muscle-sets die out at the close of the movement, we feel that the movement is passed and done; there is no feeling of suspension or pause. In the movement-cycle previously described there is but one point where normally the limb is poised; at the end of the back-stroke, and just before the next beat-stroke, records show that there is a moment when the limb is at a standstill. There may be a suspension and a poised condition anywhere in the relaxation-phase. The conditions of the beat-stroke preclude any condition of equilibrium and 'pause' in the contraction-phase. If, then, the auxiliary movements 'flow into' the movement of the accented beat, *i. e.*,

[1] *Loc. cit.*, S. 389.

if the transition from the subordinate beat to the accented beat (or *vice versa*) takes place with continually changing tensions without equilibrium occurring between the various muscle-sets involved, the subordinate movement will be continuous with the accented stroke. If at any point during the whole unit-group the conditions of muscular tension are stable and the limb comes to a stop, there will be at that place a 'pause' or suspension.

The observed structure of the unit groups (*cf.* Fig. 3) may be explained by the grouping process just outlined. In the case of the single beat, the beats are too far apart to be grouped; by hypothesis they are as far apart as heavy accents made by the slowest moving muscle-sets can occur.

The tremolo does not show a type of grouping like that of the other four unit groups. At the rate of 4–8 per sec., interval of 125–250 sig., a single member is beating as rapidly as possible; by combining two or more members as the fingers in piano playing the rate may be as rapid as c. 8–18 beats per sec., interval 125–55 sig.[1]

It is apparent that the speed with which a series of beats can be produced by a single member is about half or a little less than half the maximal speed of a series of beats produced by the combination of two or more members. In a rapid trill at the piano the first finger is raised while the second delivers its blow, and the second is raised when the first descends, so that the interval between the beats of the trill is actually the duration of a single stroke. The duration of the beat-stroke in the records taken (*cf.* Table I.) is from 50–100 sig. If the interval between a trill or a run has the duration of a beat-stroke of the record, the rate for the series of beats would be from 10–20 beats per second. The maximal rate is not determined by the number of members employed, as Reif assumes, but by the minimal interval, the duration of the beat-stroke. The beat-stroke is a process during which the judgment is very uncertain, and a coördination by which a second member shall strike a beat at the end of the first third or half of the beat-stroke is impossible. In beating a series at a maximal rate of succession, there are two and only two events which can act as clues to the reciprocating muscle-

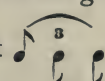
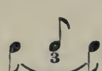
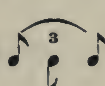
[1] *Cf.* Reif, *loc. cit.*, and Rieger, *loc. cit.*

sets: the sudden contraction of the positive muscle-set at the beginning of the beat-stroke, this is the cue for the contraction of the negative muscle-set which brings the beat-stroke to an end and throws the member back to the point of starting: second, this contraction of the negative-set becomes the cue for the positive muscle-set which catches the limb at the end of the back-stroke and throws it into a second beat-stroke. Both movements are ballistic and of nearly equal duration, if one member or two members are producing the series, but if three or more members are employed as the fingers in a piano run, the back-stroke of each member may be slower and not of the ballistic type. It is obvious that in a trill or tremolo executed by one or two members, there can be no accentuation, for an accentuation would mean an increased time to recover from the heavier contraction, and would drag into play slower acting muscles with which the rapidly acting muscle-sets could not fuse at the accented beat without being delayed. In a run accentuation may be possible because each member works more slowly, but the working of the slower auxiliary muscles is so distinct from the process producing the run that the form of an accented run is practicable only where two distinct rhythmic processes are present. It is also obvious that in the rapid succession of the trill, tremolo or run there will be continuity between the beats; they will all seem parts in the same movement, for at no point in the process is there opportunity for stable tensions and a poised condition. The tensions must always be changing rapidly and the movements 'flow into each other.'

In the case of the iamb, trochee, dactyl (and anapest) the temporal relations are determined by the events in the complex movement producing the group. There is a long interval after the accent because the heavy contraction of all the muscles concerned takes place at this point and time is needed for their relaxation. During the remainder of the group, the larger, slower muscle-sets (often these make up the musculature of the proximal segments of the limb) relax and readjust themselves for the next accent, while the more mobile sets (usually of the distal segments of the limb) beat the subordinate elements of

the group with less force and with less amplitude of movement; their action is not independent of the action of the larger, major sets on which they depend for support and control, and with which they merge at the general pulse of the accent.

If movements are prescribed for a subject, in which distal and proximal strokes of the limb are alternated, the unit-groups can be produced artificially.

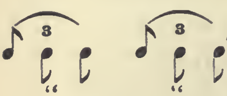
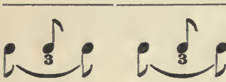
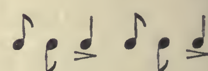
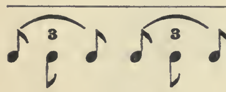
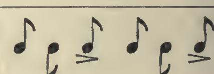
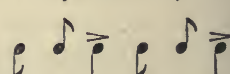
One very interesttng set of records shows the tendency of the muscles to rearrange themselves in their normal coördinations of major and minor muscle-sets in the movement-cycle of the unit-group. The form chosen was the triplet to which both hand and foot contributed beats. This form is not at all difficult when the order is foot, hand, hand, etc., , where the slow-moving muscle-sets of the foot become the major muscles giving the accent. But the combinations hand, foot, foot  and foot, hand, foot , and hand, foot, hand  are very difficult and awkward.

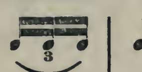
Two subjects who had had some practice in using the foot for independent rhythms were asked to beat the forms. The records of Table III. show that it is very difficult to keep the accent on a hand beat, and to keep the beats even approximately equidistant. The direction of the irregularity is important. In every case the interval from hand-beat to foot-beat is shortened, and the interval from foot-beat to hand-beat is lengthened. The hand-beat becomes a mere appendage to the foot-beat, and the foot-beat assumes its normal function of giving the accent. This tendency is carried farthest in the records of St.

The short element in the trochee is at some distance from the accented element because it occurs *after* the pulse of the accent; all the muscle-sets have given a heavy beat and some time is required before the mobile sets, though they work continuously, can produce the subordinate beat. The short element of the trochee is heavier (and therefore longer) than the short element of the iamb, because it occurs in a part of the group-

Table III.

Records of Triplets in which the Beats were made by both Hand and Foot.

Subject.	No. Triplets Measured.	Av. Height 1″ Foot-stroke mm.	Av. Interval Hand to Foot σ.	Av. Interval Foot to Foot σ.	Av. Interval Foot to Hand σ.	Av. Height 2″ Foot-stroke mm.
 Th	13	19	265	300	307	16.5 tends toward form 
" " St	14	22	274	291	322	15.5 " " " "
			Av. Interval Foot to Hand σ.	Av. Interval Hand to Foot σ.	Av. Interval Foot to Foot σ.	
 Th	8		359	337	399.5	tends toward 
	10	20	287	268	350	18 " "
" " St	7		291	268	514	has become 
			Av. Interval Hand to Foot σ.	Av. Interval Foot to Hand σ.	Av. Interval Hand to Hand σ.	
 Th	11		312	343	355	tends toward 
	16		252	286	291	
" " St	12		273	339	568	has become 
			1″ interval normal	2″ " "	3″ " "	In every case the interval hand to foot is shortened.
 Th	11		359	362	364	Normal intervals as a check.
	9		353.7	359	366	
" " St	14		189	203	248	All show a slight 'measure pause.'

movement where the muscles as a whole are more or less tense. The short element of the iamb is very light (and therefore very brief) and very near to the accented element, because it occurs *before* the accented element and is therefore in a part of the group-movement where the tensions are least (poised condition of the slower muscles just before the beat), and as there is only the relaxation from its own light movement to intervene before the accent, the interval separating it from that accent is very short. The normal dactyl has one short element in the place of the short element of the trochee, and one in the place of the short element of the iamb. (The form  is really a modified trochee in which the short element is reduplicated. In the same way, the temporal values of the anapest point to its being a modified iamb. Reduplication of this kind is very common in music (*cf.* the iamb with triple reduplication in Mendelssohn's 'Funeral March'  ·). The time values of the rhythm groups just described are capable of considerable variation. An increase of the weight of accent will tend to shove the short element of the trochee farther and farther from the accented element. The lifting of the limb to an unusual height to produce accentuation will tend to make longer the interval between the short element and the accented element of the iamb.

But the conditions in which the movements of the unit-groups will be continuous, and the group therefore a unity, limit the position of the short elements. That the movement of the more mobile muscle-sets shall be continous in the interval between the general pulse of the accent and the beat-stroke of the short element, it is essential that the muscular tensions change constantly, that the movement keep going on; there must be no time of stable tension between these more mobile muscle-sets, no 'pause,' between the accent and the short element. Thus the short elements of the unit-group appear as appendages to the main movement of the accent. The foot-pause, where there is a foot-pause, appears at the point of stable tension and poise in the relaxation-phase. The iamb and the trochee usually have such a pause. In the normal dactyl it

occurs between the two short beats, but it may be absent. The form of dactyl with the pause at the end of the foot is not strictly a dactyl but a reduplicated trochee. An *apparent* pause after the two short elements (*i. e*,, what seems a pause on the records) is sometimes due to the lifting of the limb for the next heavy accent.

The characteristic affective tone of the simple rhythms must be due to the type of movement of the unit-groups. The short element of the iamb occurs at a point in the general movement-cycle where it acts as a sort of anticipatory stimulus and serves to emphasize the accented beat; the contrast, too, between its light short- and heavy long-element is greater than that of any other group. Perhaps this has something to do with the heavy, definite, final character of the iamb. The short element of the trochee occurs during the earlier part of the relaxation process at a point where it must stimulate this relaxation, and tend to shorten the total group-movement.[1] This may be a clue for the reason of the bouyant, non-final character of the trochee. The dactyl has all the movements possible to a simple unit-group; it is the most closely knit and most continuous of all the forms of grouping, and this doubtless has something to do with the steady, rapid, full impression that it makes upon us.

Mention has already been made of groups in which the different parts of the group were produced by different members as the fingers in piano playing, and the question arises how one is to explain the continuity and unity of movements involving separate members. An easy way out of the difficulty would be to assume a rhythm-apparatus of invisible muscle-sets which produces all rhythms and directs the action of separate members engaged in the production of simple rhythm-groups. Certainly there must be such a mechanism when we listen to a sound series and rhythmize it. While one cannot deny that such a method of directing separate members is possible, it seems more probable that all muscle-sets, whether or not they happen to belong to segments of the same limb may produce movements which are continuous to consciousness, and which therefore come directly to a unity of movement without the intervention

[1] *Cf.* experiments of Cleghorn, A., *Am. J. Phys.*, '98, 1, p. 336.

of another process. We have such unified movements often in non-rhymithical experience. Speech, riding a wheel, walking, are examples of such unified movements involving different members. The unity of an act seems to depend on the continuous character of its constituent movements and on the purposive habit which gave rise to it, rather than on the anatomical relation of the parts involved.

3. Perceived Rhythms.

Thus far mention has been made of rhythms only as they are produced; it remains to consider the process by which the rhythm-movement is induced in the act of perceiving rhythms. Every sound must be assumed to cause a movement involving the general musculature; if the series of sounds occurs at regular intervals, the movements will take on the coördination which answers best to the objective form of the series. The slower muscle-sets roused will adjust themselves to beats occurring at convenient intervals, and the intervening beats will occasion contractions of auxiliary and more mobile sets which thus produce the subordinate beats of the unit group. In the establishing of this coördination, the occurrence of a sound slightly before or after one of the pulses of the incipiently coördinated movement will not destroy the coördination,[1] but will alter it slightly.

In case the sound which falls on the accented beat does not have an intensity corresponding to the intensity of the beat, the movement-cycle itself contributes the requisite vividness. This is one of the most common illusions occasioned by rhythm. This universal 'subjective accentuation' goes to show that in the rhythmic perception the vividness of the various beats heard is determined by the motor process at the basis of the rhythm-perception. The only way that the subjective character of the phenomenon can be verified is by shifting the accent from point to point in the objective series, as can be done at will in many cases (ticking of a clock, metronome, etc.).

Not only the degree of vividness of the different beats, but

[1] Cf. Hofbauer's Zusch experiments, *loc. cit.*, and 'Rhythm and Rhyme,' *loc. cit.*, p. 457.

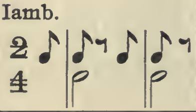
also the position in the objective time series in which the sounds occur may be modified by the rhythm process. This 'temporal displacement' has often been noted. Its explanation is important to a thoroughgoing theory of rhythm. If a series of equidistant sounds like the ticks of a metronome are heard, the observer may phrase them into iambs, trochees, or dactyls, as he chooses. To the ear, the choice seems to affect merely the vividness of the beats. But if the observer taps a key at each beat of the metronome, it will be found that in the case of the iamb he always misplaces the beats; often the same thing happens in the case of the trochee and dactyl. Because of the influence of the rhythm process, the observer does not hear the beats where they actually occur. In the case of the iamb he always hears the beat which he makes subordinate nearer the accented beat than it actually is in the objective clicking of the metronome. Table IV. is made up from the records of two subjects who listened to the equidistant and uniform sounds of a metronome grouping them either as iambs or trochees, tapped a key with each beat of the metronome, and beat with the foot once during each iamb or trochee. All three series were recorded.

In the case of the accented beat of the iambs, it will be seen from the table that the three events, the sound, the tap of the

Table IV.

Temporal Displacement in Perceiving Uniform Sound-Series.

Record of tapping with the hand in unison with a series of equidistant clicks which the subject grouped in different ways. At the same time the foot beat once for each unit-group. The objective sound series, the tapping of the hand and the beating of the foot were all recorded.

	Subject.	Objective Interval, σ	First Interval, σ as Tapped. 1	Second Interval, σ as Tapped. 2′	From Unaccented Tap of Hand to Sound, σ.	From Accented Tap of Hand to Sound, σ.	From Sound to Beat of Foot, σ.	Metronome Tempo.	
Iamb.	Th.	c. 1000	870	970	32	0	0	60	Unaccented tap held down until back-stroke of foot.
			890	1050	16	0	26		" "
			520	1030	26	—15	13		" "
			970	1030	26	0	0		" "
			1030	940	0[1]	9.7	32		" "

					From Beat of Foot to Accented Tap of Hand.	From Accented Tap of Hand to Sound.		
Th. Iamb. as above.	c. 1000	820	1000	26	32	42	60	
		900	1030	58	0	0		
		900	950	58	10	13		
		1000	1100	0[1]	32	29		
		1020	970	42	36	65		
		1060	940	0[1]	64[1]	71		

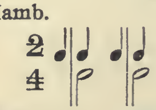
[1] Coördination fails.

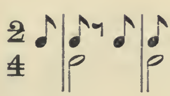
					From Accented Tap of Hand to Sound.	From Sound to Beat of Foot.		
Th. Iamb. as above.	c. 480	420	468	42	0	26	120	Unaccented tap held down until back-stroke of foot.
		420	548	29	19	16		" "
		420	542	32	10	16		" "
		420	529	32	32	29		" "
		484	548	0	13	23		
		452	516	48	0	26		
		462	542	13	0	32		
		462	484	32	32	16		
		452	548	16[1]	42	0		
		484	484	0[1]	0	45		
		484	497	0[1]	0	32		
		478	462	16[1]	0	6		

[1] Coördination fails.

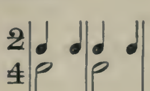
While the order in the case of the unaccented tap is invariable, the order at the accented tap is variable.

Trochee records of *Th.* do not show displacement.

					From Beat of Foot to Sound.	From Sound to Accented Tap of Hand.	
Iamb. Bg.	c. 1000	952	1030	16	6	61	60
		870	1130	13	0	97	
		823	1064	32	32	96	
		807	1064	32	16	80	
		807	1130	16	3	129	
Same. Bg.	c. 1000	871	1096	42	—19	0	
		897	1064	32	—13	39	
		913	1030	32	—13	45	
		936	1050	0	3	22	
		871	1064	0	32	64	

					From Beat of Foot to Accented Tap of Hand.	From Accented Tap of Hand to Sound	
Iamb. Bg.	c. 370+	210	403	90	26	19	160
		210	461	76	19	10	
		220	420	97	19	13	
		226	429	68	0	45	
		258	452	42	19	29	
		274	484	48	16	35	
		258	420	32	0	42	
		258	452	52	0	45	
		274	484	58	16	32	
		245	419.5	81	16	29	
		258	516	52	23	39	
		258	516	90	32	32	
		258	497	94	55	29	
		258	478	81	19	45	
		291	484	65	22	42	
		291	529	45	13	13	
		258	465	87	35	48	

					From Beat of Foot to Sound.	From Sound to Accented Tap of Hand.	
Bg. Iamb. as above.	c. 340	275	285	10	25	0	160
		300	355	20	45	0	
		300	400	15	40	0	
		300	430	50	50	0	
		300	380	60	55	5	
		285	350	50	50	0	
		340	385	0	70	5	
		300	350	35	20	5	
		325	400	0	30	0	
		325	400	35	50	10	
		320	430	45	45	5	
		370	360	50	50	50	
		320	415	25	0	40	
		315	450	45	50	50	
		280	395	90	50	25	
		280	370	75	40	10	
		325	340	20	30	20	
		325	375	10	90	10	
		315	325	50	50	35	
		275	300	50	75	15	

Subject.	Objective Interval.	First Interval, as Tapped. 1′	Second Interval, as Tapped. 2	From Beat of Foot to Sound.	From Sound to Accented Tap of Hand.	From Unaccented Tap of Hand to Sound.	Metronome Tempo
Bg. Trochee	c. 325	200	370	0	25	15	180
		225	300	15	50	25	
		300	400	20	15	25	
		270	365	30	25	0	
		220	400	25	45	40	
		250	425	0	50	15	
		240	400	0	75	0	
		200	400	5	50	40	
		315	440	15	0	20	
		225	350	0	90	25	
		290	365	0	10	40	
		300	360	0	0	25	
		300	415	10	0	25	
		365	400	0	50	25	
		365	375	5	0	40	

Other records of Bg. show trochee grouping without displacement.

finger and the beat of the foot seldom coincide exactly. Often the beat-stroke of the finger seems to continue the beat-stroke of the foot for a few sigmas. But the metronome sound always falls somewhere within the contraction-phase, usually toward the close of the limiting sensation of the hand or foot or both. The sound at the accent *never* occurs as early as the beginning of the beat-stroke of the foot. But it is very different in the case of the subordinate beat. Here the sound *always* occurs before the tap of the hand, and it is mainly this delay of the tap at the alternate unaccented beats which produces the difference of the intervals; it is here that the shifting, the 'temporal displacement' occurs. Figure 6 illustrates the relation between the actual series and the perceived series.

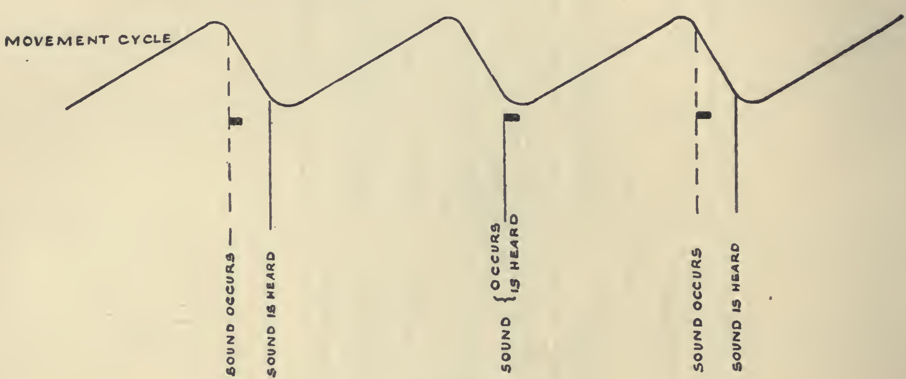


FIG. 6.

In reviewing the records at various tempos of both subjects, it will be seen that the amount of displacement is about the same at all rates. It is not possible to shift the accent more at the rate of 1 beat per second than it is at the rate of 3 beats per second, although the effect of the same amount of displacement is much more apparent in the case of the rapid tempo. This displacement at the subordinate beat varies from about 25 to 100 sig. As the sound *precedes* the tap of the hand by this amount, it must be that the sound comes at or just after the beginning of the beat-stroke of the finger, whatever the interval between the sounds. The conclusion is inevitable in the case of the groupings given in Table III., that a sound heard during

the beat-stroke is referred to the end of the beat-stroke and becomes a part of the limiting sensation. There are other familiar phenomena which indicate that this reference of a stimulus occurring during the beat-stroke to the limiting sensation at the end of the stroke is a universal tendency. In jerking a bell-rope, or tapping a gong with the foot, the tension of the cord and the impact of the foot on the lever seem a part of the common limiting sensation which includes the sound; in striking a piano or typewriter key the pressure sensation at the impact with the key is not distinguished from the resistance at the bottom of the stroke; in fact, the feeling of impact and the sound seem all to occur at the bottom of the stroke, although it often happens that the first impact occurs at or before the middle of the beat-stroke of the finger (or hand). Both subjects of Table III. succeeded in grouping the clicks into satisfactory trochees without a definite displacement. In the one case recorded of Bg., where the trochaic grouping shows displacement, the sound precedes the limiting sensation of the hand at the accent, and the whole process seems to be reversed.

4. *Question as to the Priority of the Different Unit-groups.*

A number of discussions have centered round the question as to which unit-group occurs first historically.[1]

When one considers that all the types of groupings were developed in various acts long before the sense of rhythm appears, the unit-group can hardly be said to have a development. The particular type of grouping which first becomes coördinated and produces the rhythm-experience probably depends on chance and varies with the individual. The ordering of the unit-groups into larger unities is possibly a matter of historical development, and might be studied in primitive art-works.

5. *Groupings Larger than the Unit-group.*

It is doubtful if the normal unit-group ever occurs when it is not part of a larger whole. By experiment it has been shown

[1] *Cf.* Riemann, H. Musiklexikon, 94, where the three-group (dactyl) is derived from the two-group (trochee).

Squire, Mrs. C. R., 'A Genetic Study of Rhythm,' *Am. J. Psy.*, '00, 12, p. 536.

that the coördination of the rhythmic movement is not perfect until two or three groups have been produced.[1]

It is very hard to produce a series of unit-groups without falling into some larger grouping. In poetry the simplest combination of unit-groups is the verse, or in the case of long verses, the verse-section. It is uncertain, however, if the verse-section differs from the verse; probably the verse divided into two sections is but the combination of two separate verses. Verses, again, are combined into stanzas, and stanzas occasionally become parts of a poem with a definite form as a whole. In music, the phrase corresponds roughly to the verse, and the period (strain) to the stanza; the movement made up of periods is the analog of the poem with a form as a whole.

The most obvious characteristic of the phrase—as it will be convenient to call the larger unity of the type of the verse—is the number of accents or unit-groups which occur in it. This is the basis of the ordinary classification of poetic verses and the classification of musical phrases by the number of measures they contain results in the same thing. A second characteristic is the pause which occurs in most cases at the end of the phrase. The end of the phrase is also marked by certain anticipative and recurrent factors like rhymes, refrains, and related pitch intervals and harmonies in music. One of the most important characteristics, though not indicated by the ordinary notation, is the dynamic form of the phrase. The phrase rises to some one loudest, or most intense point which is the climax, and then dies away to the end.[2]

The process which gives rise to the phrase-unity differs essentially from the process which gives rise to the unit-group. The attempt to make the unit-groups subordinate to one unit-group, as the short elements are subordinate to the accented element in the unit-group, or to build a new 'rhythm' out of the phrase-pauses or other recurrent factors overlooks important characteristics of the phrase. The recurrent factors are quite too far apart to come within the minimal interval for rhythmiza-

[1] Rhythm and Rhyme, *loc. cit.*, pp. 421 and 455.

[2] *Cf.* Wallin, *loc. cit.*, p. 136, 'Rhythm and Rhyme,' *loc. cit.*, 446–7 and 455–6 (5).

tion.[1] Moreover, each phrase is separated by a pause of indefinite length which is not a function of any of the duration values of the unit-group. Two beats alone of a single-beat rhythm could not give a satisfactory sense of rhythm, and yet the couplet is a satisfactory form of stanza.

There are many reasons for considering the phrase as simply the form of a single act, a movement composed of several subordinate movements. The tensions of the muscle-sets do not cease until the end of the phrase. The dynamic form of the phrase is the form of a movement; there is a rise to the central point of effort and then a decline at the end. Any elaborate, rapid flourish made with a pencil, or with a finger in the air, will show just such dynamic variations. When producing actual rhythmic phrases, it is easy to beat them as one complex movement; starting at an elevated point with the baton, one may beat a series of unit-groups which form a phrase as the arm descends, and it is with a feeling that the movement is *done*, and that the arm may relax, that one reaches the end of the phrase. In reciting verse, or in singing, a phrase becomes a single act of expiration; indeed, just this movement of breathing is probably the origin of musical phrasing.

If there is a pause during the phrase, within or between the unit-groups, there is always a feeling of tension of which the subject becomes quite conscious if the pause is prolonged,[2] but at the end of the phrase, there is a release of these tensions, as if the coördination of the muscle-sets were at an end. In records of scanned nonsense syllables this dying out of the tensions at the end of the phrase is very striking. Every final syllable of the phrase ends in a long decrescendo, showing that the tension of the expiratory muscles dies out gradually at that point. The phrase pause is indefinite and non-rhythmical because it is simply the time necessary for the movement to die out.[3]

The way in which the phrase is brought to a close is of interest, because it is evident from experiments[4] that the form of the coördinated movement of the phrase may be so indefinite

[1] Cf. Wallin, *loc. cit.*, p. 72.

[2] 'Rhythm and Rhyme,' *loc. cit.*, 421.

[3] *Ibid.*, p. 447.

[4] *Ibid.*, p. 419.

that it is not determined until the end of the phrase; and in establishing the coördination when one listens to a new phrase, it is evident that the form is not determined until the close of the phrase. A mere pause during which the tensions die out gives the movement a somewhat indefinite character, and very frequently the last elements of the phrase, or of the larger unities, are varied so as to make the end definite. The sense of finality is often produced by a heavy accent. This accent is often determined by some qualitative factor which attracts attention and receives emphasis. The heavy accent for finality, with a long dynamic preparation is especially noticeable in musical forms, and in some poems which 'end with a climax.' In the striking of any blow, the event, the point of the whole movement, is connected with the resisted momentum at the impact. The heavy impact at the close seems not only to mark the event, but to release quickly all the tensions of the movement; all the tensions of the movement seem to culminate in this effort and then disappear. It may be that the heavy blow upsets the coördination; it may be that the remaining energy of the process is all consumed in the final effort; it may be that there is some reflex connection between heavy contraction and relaxation.

When it comes to unities larger than that of the phrase, the movement theory can give no help. It is apparent that the connection of verses in stanzas, or of phrases in periods is unlike the connection of unit-groups in the phrase, and that the unity of the stanza is not the unity of a single movement. Each phrase produces a total impression, a 'Gestaltsqualität,' which appears only at the end of the phrase. It is a memory image of the 'motor' type, often associated with the appearance of the material on the page. Somehow this set of strains, this memory image, becomes to some extent the cue of the movement of the following phrases and may modify their character. Rhyme, and melodic and harmonic relations play an important part in this Gestaltsqualität. But as for the actual connection of the phrases, and the unity of the period, one can only say vaguely that somehow the effect of the phrases is carried over, and a set of strains aroused which are satisfied by the subsequent phrases, and the conditions of this satisfaction are the conditions of the

period form. One may imagine that the process is not unlike any process of obtaining an æsthetic unity, as in the composition of a picture, or the structure of a drama.

6. *The Rhythm of Verse and of Prose.*

There is no reason for assuming that the nature of the unit-group of verse differs from that of other rhythms. Some of the recent laboratory work dealing with verse rhythm has been based on the conception of the unit-group as centering about a 'point' determined by three factors, force (loudness), length, and (relative) pitch.[1]

A motor theory involves the association of force and length; and pitch, to be sure, is physiologically a matter of muscular tension. But it is not therefore a factor in the movement-cycle of the unit-group. In the first place, Scripture's conception of the function of pitch is based on a mere logical analysis of the properties of an uttered sound. The sound has amplitude, duration, and pitch, therefore the 'centroid' will be determined by these factors. Quite as good a case can be made for quality; changes of pitch frequently occur on accents, so do certain types of quality which have a demonstrable part in the rhythmization of verse, such as alliteration, assonance and especially rhyme. But whether either quality or pitch is a factor in the unit-group must depend on the nature of the movement-cycle, not on the occasional association of these phenomena with the unit-group. Pitch is due to the tension of the muscles of the vocal cords, and the tension appears in consciousness, but the muscles of the vocal cords are not capable of the reciprocal movement, with the sudden blow, necessary to muscles which take part in the rhythm-cycle. If the vocal cords participated in the blow of the rhythmic beat-stroke, the oscillation of the pitch during the enunciation of an accented syllable would be extremely wide, instead of being as a rule very slight. And there could never be a falling of the pitch on the accent, as the advocates of the pitch factor seem to assume. The voluntary tension of the vocal cords seems to remain about the

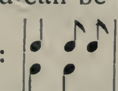
[1] The 'centroid,' Scripture, E. W., 'Researches in Experimental Phonetics,' *Stud. Yale Psy. Lab.*, 99, 7, p. 101: Wallin, J. E. W., *loc. cit.*, p. 9.

same during the utterance of an accented syllable, and in singing this evenness of pitch is a requisite. There is a mechanical reason for a slight change of pitch during the utterance of an emphasized syllable; the increased pressure of air against the vocal cords must increase their tension and tend to cause a slight involuntary rise and fall of pitch during the syllable.

If pitch were a possible constituent of the unit-group, we should certainly expect to find it in use in music, especially in vocal music always so closely associated with poetry. But in spite of the pitch material at hand, it is never used as a means of accentuation in the mere unit-group in any form of music, nor does music show any hint of compensating for an accentual effect of pitch. The same intervals are used in close succession on accented and unaccented elements, and the possibility of an influence on the mere grammatical accent has never even occurred to the musical theorists. In much the same fashion as in music there are melodies of speech, with intervals which are handled quite independently of the unit-groups, and which are never determined by them.

Length and intensity (loudness) figure in verse rhythm as they do in any rhythm. Intensity is the fundamental property of accent and marks the definite event in the movement-cycle. Since the movement-cycle is a cycle, and each unit-group though it may differ in details, must seem like the preceding group, there are certain temporal relations which can vary only within limits. As to individual syllables, Gurney is quite right in asserting that the accented syllable is longer because it is uttered with more force (*i. e.*, with a movement of greater amplitude and involving more muscles). All verse is therefore quantitative, and all verse is accented. The distinction between classical and modern verse based on quantity *vs.* accent is a mistake. Recent laboratory work has shown that quantity is a characteristic of the syllables of ordinary speech and of a single syllable repeated rhythmically.[1] And mere quantity without accents cannot constitute a rhythm. The quantitative theory of pronouncing Latin, advocated by Hale and others, is in a peculiarly unfortunate case, as it is wedded to a ratio of

[1] Wallin, *loc. cit.*, p. 32. 'Rhythm and Rhyme,' *loc. cit.*, p. 443.

1 : 2 between short and long, and that particular time relation is a hard one to maintain, even with the assistance of rhythm. As to the exactness of the time values of the classic poetry, everything depends on the character of its reading. If it was read, as is possible, as a musical rhythm, *i. e.*, as a combined, not as a single rhythm, then as exact temporal relations as in music may have been maintained. That the ancients did not chant their poetry with a silent or visible accompaniment cannot be categorically denied. Such an accompaniment would furnish the secondary series of a combined rhythm. But such a combined rhythm in reciting poetry seems very improbable for it would hamper any attempt at expressiveness. It is useless to appeal to the 'delicate rhythmic sense of the ancients,' and assume that they got on without such an accompanying rhythm. The rhythmic training of the modern musician is indefinitely better and broader owing to the modern development of musical rhythm, and yet he cannot get on without the secondary rhythm; he has always to 'keep time.' The familiar dactylic hexameter is a good illustration of what the quantitative theory involves. The foot is supposed to have the form |♩ ♪♪| or |♩ ♩| and it is usually explicitly stated that it is to be given in two-four time.[1] The two-four form |♩ ♪ ♪| is not a simple rhythmic group, and can be given only with an accompanying takt-rhythm, thus: . Such a beating of time, two beats to each foot, would have made the delivery exceedingly labored, whether the verse was read 'as in singing' or not, and it is beyond belief that any poet ever recited his verse in such a fashion. The reference to singing in support of the quantitative theory does not help, for accent is as necessary in singing as in any other type of rhythm. In the free delivery of speech, even in singing, a secondary rhythm is not present. Everyone knows that in recitativo singing, the time is not kept, *i. e.*, one does not beat the secondary accompaniment. It is very doubtful if the utterance of the Greek bard was more rigid than that of the singer of recitative in the modern oratorio.

[1] Greenough and Kittredge, 'Virgil's Æn. and Bucolics,' '99, p. xxxviii.

It is worth pointing out as against the duration-loudness-pitch theory of the unit-group of speech, that mere duration is not a factor of intensity or of vividness. The long duration that seems significant is the duration of an accentual movement and nothing else. A lag introduced into a verse just after an accented or unaccented syllable does not increase the intensity of the syllable preceding, whether the sound of the syllable is prolonged throughout the lag or not. A special case, in which the lag will increase the intensity of the *following* syllable is significant. During the lag the movement of the back stroke tends to continue, and at first seems to lift the apparatus higher, and thus gives a longer, more intense stroke to the next beat. If the lag continues past this point, the tension seems to die out, and the next beat is less intense than normal.[1] And in music the prolonged note is not therefore an emphatic note.

Loudness is a factor in the accent of verse rhythm, because in reciting poetry the loudness represents the intensity of the movement. When we listen to verse, the more intense sounds rouse a greater motor reaction, because of the reflex between the ear and the muscular system, and because speech usually calls up a response in our own vocal organs. When one reads verse silently, the movements are probably movements of incipient articulation in the vocal apparatus. No succession of images which are not accompanied by actual movements can give rise to a rhythm.

The poet finds his material ready-made; his rhythms are mosaics of bits of movement already stereotyped in speech. Each word has the structure of a unit-group or collection of unit-groups with the consequent relations of accent and time. But the words of ordinary speech are not coördinated into a rhythm. The bits for the mosaic must be selected so that a regular pattern results, so that the movements will have the regularity and the form of a series of unit-groups.

An analysis of the movements of ordinary accentual speech will show the material of the unit-group of verse.

In the vocal apparatus the musculature of the cords contributes tensions, but does not enter into the movement-cycle. Move-

[1] 'Rhythm and Rhyme,' *loc. cit.*, p. 427.

ments with a rapid stroke are possible to the lips, jaw, tongue, palate and to the diaphragm in coördination with the abdominal muscles; and possibly to the chest-walls in coördination with the diaphragm and abdominal muscles. The movement cycle of the muscles of speech differs slightly from that of the limbs. In most cases the positive muscle sets contract and tensions rise, some time before the sudden release which makes the beat-stroke. The action is much like that of snapping the fingers, when the contraction of the positive muscle-set occurs at first with middle finger and thumb pressed against each other and acting in opposition; when the release comes the beat-stroke has a much higher velocity than otherwise possible, though the length of stroke is still under control. In speech the air is confined in the lungs until the contraction of the abdominal muscles (*et al.*) has produced the tension, then the outlet is opened and the beat-stroke occurs. The vowel at the beginning of which the beat-stroke occurs has an 'explosive' quality. At the end of the beat-stroke, either the sudden contraction of the diaphragm (*et al.*) acts as the negative muscle-set and checks the ballistic movement, or the closing of the air passages by the next consonantal movement produces the negative resistance. The movements of consonants preceding vowels often serve to close the air-passages and raise the tension just before the beat-stroke. Where the consonant is not vocalized, as *k*, *t*, *f* and *p*, the closure is complete and the release is very sudden and the beat-stroke takes place at the consonant. Where the consonant is slightly vocalized, as *g*, *d*, *n*, *m*, *b*, *v*, the beat-stroke occurs in slow speech slightly after the beginning of the consonant. In the case of liquids and aspirates there is no preliminary rise of tension; the beat-stroke occurs at the beginning of the consonant, and the sound of the consonant continues into the vowel. When consonants follow the vowel, the impact of the consonant occurs at the end of the beat-stroke; it sometimes happens that the position of the beat-stroke (the expiratory pulse) determines the grouping of consonants otherwise alike. 'This train' and 'this strain' are distinguished in ordinary speech by the fact that in the word 'train' the beat-stroke comes on the 't.' In the word 'strain' on the 'st.' In the case of a consonant pre-

ceding a vowel, if the impact is not prolonged, the consonant seems a part of the beat-stroke. If the vowel is very brief, the consonant following the vowel may appear at the close of the beat-stroke of which it becomes a part; otherwise it will occur

DIAGRAM OF BEAT STROKES IN SYLLABLES WITH CONSONANTS.

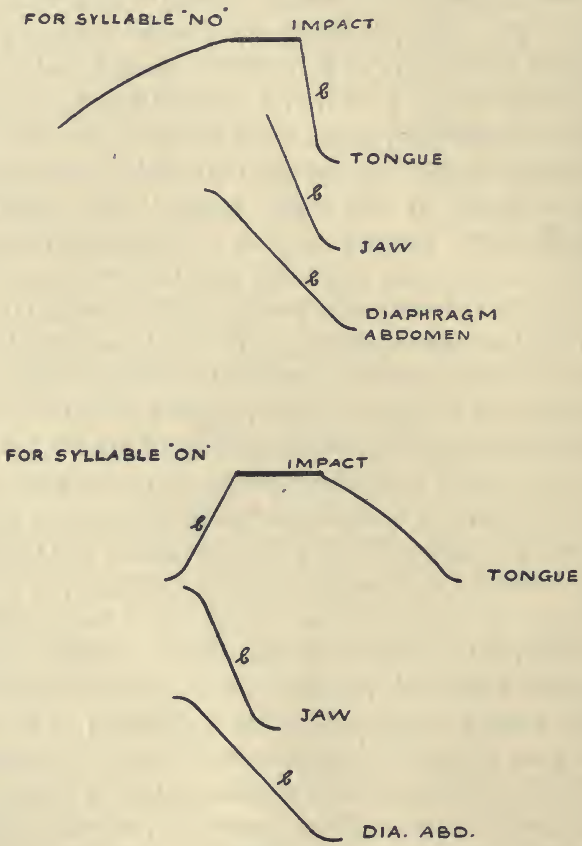


FIG. 7.

in the relaxation period. For the various relations of the consonants and vowels, compare the accompanying diagram, Fig. 7.

In ordinary speech, the length of the staccato vowel is the length of the beat-stroke. It is possible, however, for a residual tension of the positive muscle-set to maintain the tone during the relaxation phase, until obstructed by the next consonantal

movement. (This residual tension may be nothing more than the mechanical recoil of the respiratory muscles.)

The material of the unit-groups has much to do with their effect in verse. Alliteration and assonance give the unit-groups identical movements, and serve to make the rhythm more regular and more exactly coördinated. Their effect on the verse, then, is more than merely 'qualitative'; they directly affect the rhythm.

In the determination of the larger unities, the verses and the stanzas, there are a number of important factors which must not be overlooked. Each verse has a dynamic form, and its coördination with other verses is dependent on this dynamic form, which is simply the form of the phrase-movement. In the same way there are pitch relations which may give the verses a good or bad melodic relation. Dynamic and melodic relations enter into the phrasing of verse quite as really and quite as definitely as they do into musical phrasing, and much of the beauty of verse depends on these factors.

In rhyme, the relations of pitch are limited, if the rhyme is to be heard.[1] The rhyme determines a dynamic form with the climax at the rhyme, but it makes the verse-pause optional. Very elaborate and definite stanza structures are possible with the aid of rhyme, and almost all the modern stanza forms are dependent on it. Without rhyme the dynamic form of the verse is variable, but a pause at the end of the verse is an essential, if the verse is to stand as a phrase.[2]

It is evident that the rhyme functions as one of the factors in coördinating phrases, but the nature of the rhyme impression is an enigma. The beat-stroke must be the same, but the consonant at the beginning of the beat-stroke must be different; an identical beat-stroke with different consonants at the end of the stroke is not rhyme; a vowel without a consonant will rhyme with one preceded by a consonant. The best one can say is that the rhyme impression is somehow dependent on the manner of initiating the beat-stroke.

Dynamic and melodic relations of the verse are often deter-

[1] 'Rhythm and Rhyme,' *loc. cit.*, p. 430.

[2] *Ibid.*, 449 and 464.

mined by the meaning of the verse, but this does not mean that the 'content' is a factor in the rhythm. Many variants of a given rhythm are possible. Which one of these forms is chosen may be determined by the 'content,' but each one must be rhythmic in itself. The fact is that in selecting his material for a verse or stanza the poet must find material which in all its aspects will fit into the pattern of his rhythm. He cannot put a certain rhythm and a certain 'content' together and hope for a composite effect in which a 'logical coördination' plays a part in the rhythm. Any rhythm is a rhythm independent of any meaning its material may convey. It must remain a rhythm with all the lags, pauses and variations that serve the purpose of making the meaning clear, when it is stripped of all meaning. The rhythm of a foreign language is in nowise dependent on the meaning of the language.

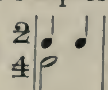
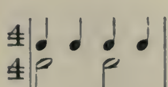
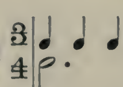
Although the words of ordinary speech show the grouping of the unit-group, the groups do not occur regularly enough so that a coördinated movement from group to group is established. In the sense of having regular unit-groups, no prose can be said to be rhythmic. The rhythm of prose is only a matter of the coördination of the phrases into larger wholes. The general movement of each clause or sentence in prose is similar to the movement as a whole of the rhythmic phrase, and like the rhythmic phrases, these clauses or phrases can be grouped together. The songs of birds and animal calls and well-written prose may all have phrases grouped into periods, and to that extent may be said to be rhythmic; but the regular succession of unit-groups is not present.

The most elaborate investigation of speech rhythms published is that of Wallin.[1] The material used was the fully developed art-work. While one may not agree with the theories advanced, the work is suggestive, and many interesting things appear. The method, however, necessarily limits the fruitfulness of the investigation in some directions. The statistical study of the poem, a complex art-form, is valuable for description, but can seldom furnish material for explanation. There is need of painstaking work with artificially simplified material

[1] *Loc. cit.*

to determine some of the most important principles of the single rhythm in verse. We do not know the conditions for the substitution of one rhythmic group for another which occurs so often in modern verse. There is little known of the characteristics and conditions of the movement of the verse as a whole, and there is nothing known of the larger unity of the period. The recording of verse rhythms for study presents many difficulties. If possible, the measurements should be visual. The method used by Wallin of determining the limits of syllables by sound is open to error both because of the continuous confusing noise which the stylus of the phonograph makes in rubbing against the record and because of the personal equation. Judgments of sound intensity based on hearing are of little worth. Subjective accentuation plays too large a part in such judgments. A record capable of reproduction and at the same time of visual measurement should be the best, and the microscopic method of study is certainly superior to mechanical enlargement, if error is to be avoided. But at best any record at present possible involves the use of a diaphragm, and the resonance of a diaphragm is always a source of error. Visual measurements of amplitude can only be made with identical vowels, as every vowel has its own amplitude of vibration. Pitch relations play a part in the phrasing of verse, but phonographic records of pitch are not reliable and are peculiarly tedious to study. For an accurate study of the conditions of verse rhythm, material adapted as far as possible to the limitations of the phonographic method must be used.

IV. Combined Rhythms.

Music with its elaborate and complex figures and its developed notation is the field of combined rhythms. The simplest types of combined rhythms are the simple measures ,  and . All other measures are recognized as a development of these forms, and most theorists derive the four-four measure from the two-four and consider the double and

triple divisions alone the basis of music; the unquestioned two-four character of the six-eight measure and the sporadic or even doubtful existence of the five-four make this seem probable. The more complex measures are built up by making a group represent a single division of the two-four or three-four measures.

The dynamic properties of these measures are well known. Accent on the first beat in the two-four measure; accent on the first, and a secondary accent on the third in the four-four measure; and accent on the first with a possible secondary accent on the third in the three-four measure; these are the familiar dynamic forms.

1. *Nature of the Measure.*

It should hardly be necessary to point out the essential differences between this new type of unity and the unit-groups of the single rhythm. Nevertheless they are often confused. The unit-groups are composed of beats having striking differences of accentuation, and wide variations in the length of the intervals, and in all the normal forms (except the single-beat and the tremolo) the intervals are essentially uneven (*cf.* fig. 3, p. 272). The measure on the other hand, has but slight differences of accentuation, the time values vary little, and the intervals are practically equal. Most fundamental difference of all, the *measure is always composed of unit-groups.* Since the measure only appears when rhythms are combined, its raison d'être must be in the movements of the combined rhythms. Although the movement-cycles of combined rhythms must be distinct and comparatively independent (as distinguished from the action of the major and minor muscle-sets in the single rhythm) the processes must influence each other, however slightly, if they are simultaneous; and one might hope to find in the primary series of a combined rhythm some traces of the movement which makes the measure unity. In records of rhythm, the factor which shows slight influences the most readily is the length of the beat-stroke. In Table V. averages are shown of the length of the beat-strokes in the beating of simple measures. In every case the lengths of the beat-strokes show a gradation in the case of a measure, a gradation in keeping with the observed accentual relations within the measure. Such

TABLE V.

RELATIVE LENGTHS OF THE BEAT-STROKES IN VARIOUS SIMPLE MEASURES.
Actual strokes 4–6 times (mm.), the values given. Beating with a Baton.

	Subject.	Average Length Stroke of 1st Beat	Average Length Stroke of 2d Beat.	No Measures of Record.	Metronome Tempo.	
2/4 ♩ ♩	Bi.	87.0	82.5	10	80 M.	Very regular, each measure shows the difference. No accent prescribed.
" "	Bg.	57.3	37.7	23	145 M.	Very regular, each measure shows the difference. No accent prescribed.

	Subject.	Average Length Stroke of 1st Beat	Average Length Stroke of 2d Beat.	Average Length Stroke of 3d Beat.	No Measures of Record.	Metronome Tempo.	
3/4 ♩ ♩ ♩	Bi.	71.0	43.5	41.3	6	120 M.	Regular ; variation small.
" "	Bg.	60.3	51.3	47.1	7	88 M.	Regular ; variation small.
3/4 ♩> ♩ ♩	Bi.	73.8	53.4	52.1	8	85 M.	Accent prescribed on 1st beat. Variation small.
3/4 ♩ ♩ ♩	Bi.	83	32	30.3	15	c. 145 M.	Fast as possible. Variation small.
" "	Th.	70.2	65.6	66	15	c. 140 M.	Fast as possible. Variation small. Form differs from preceding.
" "	Th.	100.5	65.4	58.8	5	115 M.	Variation small.
" "	St.	44.4	37.1	39	9	90 M.	Variation small. In form *cf.* the above.

	Subject.	Average Length Stroke of 1st Beat.	Average Length Stroke of 2d Beat.	Average Length Stroke of 3d Beat	Average Length Stroke of 4th Beat.	No Measures of Record.	Metronome Tempo.	
4/4 ♩ ♩ ♩ ♩	Bi.	57.0	51.0	51.9	50.0	5	88	Shows secondary accent.
4/4 ♩> ♩ ♩ ♩	Bi.	68.4	47.8	44.6	41.6	5	88 M.	Accent prescribed on 1st beat. No secondary accent.
4/4 ♩ ♩ ♩ ♩	Bi.	59.3	41.8	38.4	34	15	c. 150 M.	Fast as possible. No secondary accent.
4/4 ♩> ♩ ♩ ♩	Th.	85.0	64.9	66.1	57.2	9	90 M.	Accent prescribed on 1st beat. Shows secondary accent.
4/4 ♩ ♩ ♩ ♩	St.	59.3	41.8	39.4	34.0	15	150 M.	Fast as possible. No secondary accent.

regular dynamic variations must be due to a movement, and in certain cases this movement may become apparent on the kymograph record, as in cases where the upper limit rises higher and higher or sinks lower and lower at each beat throughout the measure showing that the measure is among other things a slow upward or downward movement.

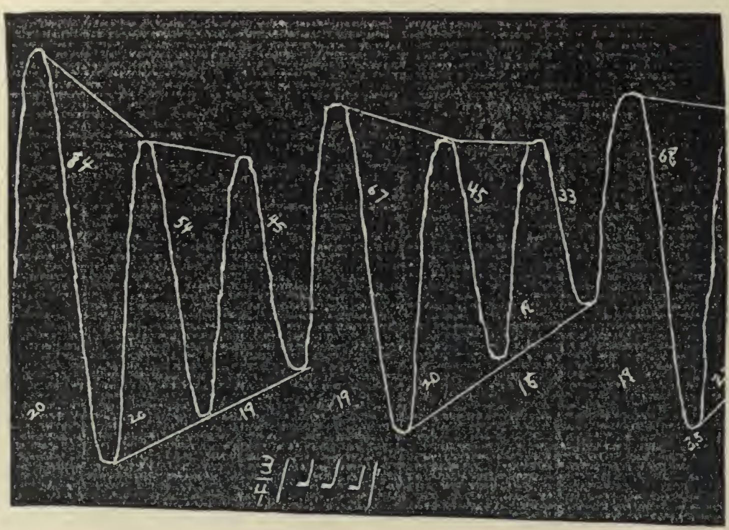


Fig. 8.

Lines have been drawn connecting the curves to show the general form of the movement.

The measure must be due to a secondary movement-cycle simultaneous but comparatively independent. The simplest possible measures may be figured as in Fig. 9. Four-four must be

Fig. 9.

considered a compound two-four measure if it has a secondary accent, as it has at ordinary tempos. The limits of the length of measure must be due to the temporal conditions of this secondary movement. The ordinary unit-group (single-beat or complex

group) does not show wide variations in duration. Miss Hallock asserts in a recent paper[1] that an analysis of the standard editions of Beethoven's sonatas shows an extreme range of 60–80 as the metronomic marking of the unit-group; the average marking for all the sonatas is 64.4.M. Well-known conductors gave 64–72 M. as the takt usually used in their conducting. (The pulse to which this author refers the rate established can have nothing to do, of course, with the length of the unit group.) It may very well be that the multiple of this value (a little less than 1 sec.) by three or four represents the maximal interval for the slower, automatic, regular movement which produces the measure.

When the measures take on elaborate forms, the rhythm of the melody or cantus firmus has the unit-groups of the types previously discussed (iamb, trochee, dactyl, tremolo, etc.). The processes in music are usually very regular, but the unit-groups often show the irregularity due to their process of production. The trochee may be of the form ♫, but when heavily accented it takes the normal form ♩ ♪, ♩. ♪, etc. The iamb is written ♫. (♫ is a false convention) but its values when executed nearly always approximate those of the normal iamb with the short element much nearer the long than the notation indicates. This cantus-firmus rhythm is the more mobile, living process of the combined rhythm; it has the type of movement of the single rhythm, and probably the same coöperation of amalgamating minor and major muscle-sets in the unit-group. It is quite possible in forms like the canon and fugue to have two such cantus-firmus rhythms running simultaneously but not in unison. In such a case it is significant that there must be a very definite takt-keeping to prevent the process falling into chaos. This takt-keeping represents a series which must be present in every combined rhythm.

2. *Characteristics of Combined Rhythms. Effect of Changes of Tempo on the Character of a Rhythm.*—It is a familiar fact that a decided change of tempo changes completely the char-

[1] Hallock, Miss M., 'Pulse and Rhythm,' *Popular Science Monthly*, '03, 63, September, 425.

acter of a musical rhythm. A waltz may become a hymn and a mæstoso processional a two-step by simply altering the rate of execution. Such changes are only noticed in music, and therefore the combination of rhythms probably has something to do with the change. On examination the change turns out to be more than a mere variation in rate. In the first place, new measure movements may appear; what had previously been three-eight may become six-eight; two-four measures may be combined to form four-four measures with secondary accent. At the same time, the takt-rhythm changes, the beats occurring less frequently as measured by the beats of the other rhythms. As already noted, the takt of music does not vary greatly in different types; it tends to remain at about the same absolute rate. Consequently, if the rhythm is played rapidly, a larger number of beats of the primary rhythm will occur during a single interval of the takt-beat. This change will affect the grouping of the primary rhythm, making trochees where before were single-beats, tremolo forms where before were trochees, etc. All these changes are radical, for they affect the grouping, and that means the form of the movements of the rhythm. The phenomenon emphasizes the fact that not temporal relations but the movements involved are the fundamental things.

In combined rhythms the temporal irregularity is very slight. An accented note is known to be longer in general than an unaccented, and at the end of the measure there may be a slight 'pause' prolonging the last beat of the measure.[1] But these variations are small and the ordinary mathematical notation of music is quite justified. How the combination of rhythms leads to such regularity is the question. In the first place, one must suppose that the movement-cycle of each beat is practically identical with the movement-cycle of its neighbors in the same series. In the figures of the simple rhythms there are beats made by a part only of the muscle sets, but in each combined rhythm there must be at least one rhythmic series in which just the same muscle-sets take part in a movement of just the same amplitude at every beat. At least one series must 'keep time,'

[1] Christiani, A. W., *Principles of Pianoforte Expression*, N. Y., '85. Riemann, H., *Dynamik und Agogik*, Hamburg, '84.

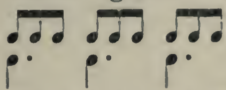
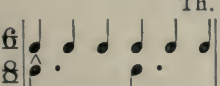
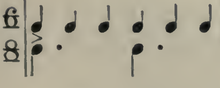
and there is but one way in the human organism to keep time, and that is to set a reciprocating movement to repeating itself without change. To some extent the processes must check each other; if the primary rhythm is executing three movements against one of the secondary rhythm, as in the form  it is evident that the primary process will serve as a guide to the secondary, and in return the secondary process will serve to keep the primary regulated, as the movements must go together and fit into each other (*cf.* Fig. 9, above). An accent in either series will tend to prolong the series at that point, but if the other series is unaffected, its movements will serve as the stimulus to the movements of the first series and hasten the relaxation-process after the heavy contraction. As

Table VI.

The Influence of Accentuation.

Effect of the two processes on each other and effect of the accent on the temporal relations.

Subject.	No. Measures of Record.	Av. Height, 1st Stroke mm.	Av. Interval (σ) During which Foot was in Air Before 1st Stroke.	Av. Interval (σ) 1st Beat.	Av. Interval (σ) 2d Beat.	Av. Interval (σ) 3rd Beat.	Av. Height, 4th Stroke mm.	Av. Interval (σ) During which Foot was in Air Before 4th Stroke.	Av. Interval (σ) 4th Beat.	Av. Interval (σ) 5th Beat.	Av. Interval, 6th Beat.	
Th. 	6	25	287.5	345.6	326.2	326.2	19	242	326	326	420.9	the foot-beat alone accented.
" "	6	21.5	339	316.5	290.7	319.8	18	258	319.8	300	339	the foot-beat alone accented.
	6	26	342.6	329.5	342.4	326	16.4	342.6	326	329	374.7	heavy accent but without directions.
" "	6	13.5	310	323	294	303	11	274	294	294	316.5	hand alone to accent.
	6	11	323	300	300	316	8.5	258	303	300	320	hand alone to accent.

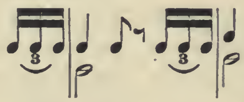
Records show that foot always has accent.

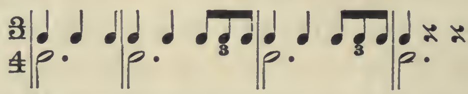
Table VII.

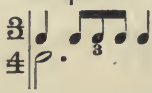
Cases Where the Rhythm of the Hand Perturbs the Rhythm of the Foot.

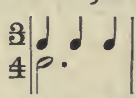
Ta. In every case where the foot is raised before the ⌣ of ∸ ⌣ and ⌣ ∸, the foot movement is perturbed. 15 cases.

Sn. In 5 cases in 11 where the foot rises before ⌣ of ⌣ ∸ the foot movement is perturbed. 5 cases in 11.

Th. In the form  the foot rises during the figure , and its movement is perturbed. 7 cases in the 9 measures.

Bg. In the form , in the measures with the figure  the foot movement is perturbed. 10 cases.

Bg. In the form  the foot movement is perturbed by the figure . 6 cases in the 9 measures.

Bg. In the form  the foot rises at the second tap of hand, and the movement of the foot is perturbed at the third tap. 4 cases in 8 measures.

Bg. Trochaic form,  = 120 M., the foot rises almost immediately after its descent, but shows perturbation at the ⌣ of the hand. 13 cases in 17.

the movements are uniform (*i. e.*, there is no amalgamation of muscle-sets at the accent, save in the melody series) the only difference between the accented and unaccented beats may be in the length of stroke which will cause but a slight variation in the process.

Although different movement processes may be executed side by side, perfect independence is not possible. Even in the case of movements which are outside the rhythm-apparatus, as in turning pages or talking while playing at the piano, an influence is noticed. It often happens that one rhythmic series is accented without the intention to accent the concomitant series. The influence of accentuation in such conditions, on the other rhythm, was studied in some detail in the case of one subject. Table VI. shows that with this subject accentuation is associated

with an increase of length of stroke, and that the foot or hand rises later after the accented stroke (*cf.* Fig. 4, p. 273). The interval both before and after the accented stroke is longer. Both processes show the accent, whatever is prescribed.

In some cases a movement not only affects another movement, but causes an actual disturbance. In Table VII. a list of such 'perturbations' are given.

They all occur during the back-stroke. They are all cases in which the primary rhythm affects the slower secondary rhythm. In every case the contraction of the muscles of the primary rhythms seems to have acted as a stimulus to the negative muscle-set of the secondary rhythm, causing a sudden deflection in the negative direction. Féré[1] mentions cases in which rhythmical movements perturb each other.

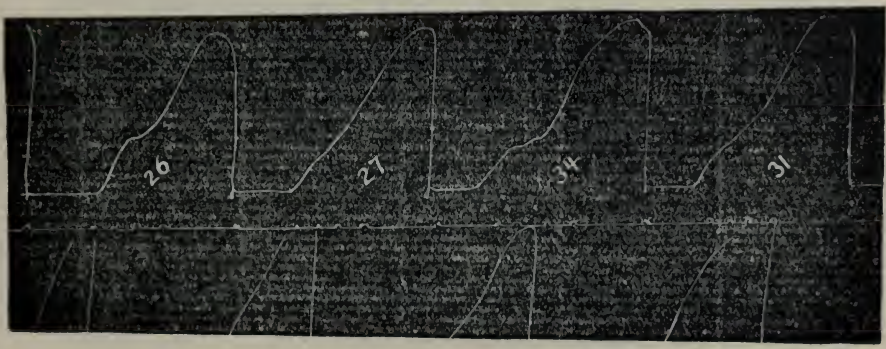


FIG. 10.

The exact relations of the superposed movements differ in different subjects. With a primary and secondary rhythm, four of the subjects, Bg., Sn., M., H., raise the foot as a rule almost immediately after the beat-stroke at all tempos; the foot is in the air throughout the whole relaxation-phase. Four other subjects, Th., Bi., Ta., St., do not raise the foot until late in the relaxation-phase. At rapid tempos they revert to the previous type, and the foot is in the air throughout the relaxation-phase.

[1] Féré, *Sensation et Mouvement*, 2e éd., reviewed in *Ztschr. f. Psy. u. Phys. d. S. Org.*, '03, 32, S. 421.

The records were inspected to determine at what point in the relaxation-phase the foot is raised in the case of Th., Bi., Ta., St. In some cases, *e. g.*, Ta., it is evident that at times the foot is raised at random and remains poised until the beat-stroke. In many other cases the rise of the foot coincides with some subordinate tap of the hand, usually with the last one in the relaxation-phase. At times it seems to occur with the back-stroke of the hand before this last tap, and at other times with the back-stroke of the hand following this last tap. There is reason to suppose that the movement of the foot is actually coördinated with this last tap, which serves, partially at least, as its movement-cue. It is probable that with many subjects the secondary rhythm is not actually produced in the muscles of the leg and foot. The secondary rhythm is probably beating in other muscles of the organism and the foot simply reacts on signal.

The two or more rhythms which combine appear in consciousness as one complex because there are certain coördinations, certain tensions which have to exist to support both processes. Just as in the single rhythm, the phrase is a movement which includes all the movements of the combined rhythms. In music there are other factors which make toward unity, but they are not essential to combined rhythms. All this presupposes a rather complex set of movements. But it is doubtful if any psychological motor scheme can surpass the actual complexity of the simplest reactions of the muscular apparatus.

3. *The Rhythm of Music.*

The striking difference between the rhythm of music and the rhythm of verse is due partly to the regularity and multiplicity of the rhythmic process, and partly to the character of the unit-groups in the individual rhythms. At first sight they present an indefinite variety of forms. Many of them turn out to be dactyls, trochees, and iambs in a variety of notations. But in addition there is a large use of single-beats, and of tremolo-groups of all rates. These tremolo-groups take the place of a single unit-group, or of an element in the unit-group, and thus add an indefinite variety to the compound forms of the unit-group. The use of tremolo and single-beat is possible

because of the regularity of the process. The other rhythms carry on the process during the single-beats, and the regularity of the recurring takt makes it possible to fill in an interval with the tremolo. At the same time, the form of the movement-cycle may be changed for various purposes. Since there is now one process at least to act as a cue to the primary rhythm, its movements may vary, and yet give the beat-stroke regularly. In staccato notes, the staccato implies a movement-cycle in which the back-stroke is made very rapidly and is followed by a long wait in the poised position before the next beat-stroke. The peculiar staccato before an accented note so often noticed in the phrasing of organ music (♪ ♩ , etc.) is very interesting. It makes apparent to the ear the unusual rise of the back-stroke for the long and therefore accented beat-stroke which follows. The device is especially important at the organ, as intensity of sound for grammatical accent is out of the question (*cf.* p. 283). In the case of single-beats the note may be 'held' through the time of several unit-groups. The other series keep the whole process alive, and are a constant stimulus for the maintenance of the tensions of the note held, and the series in which it occurs does not lose its identity, unless the note lasts through an entire section or period (organ-point). 'Rests,' which imply that the muscles of one movement remain poised for an appreciable time, become possible while the other movements go on. Interest centers in the cantus-firmus rhythm, and the continuity and activity of that particular rhythmic process are usually provided for. Legato which insures a tension throughout the unit-group is often used, and where legato is impossible as in instruments of percussion other devices are resorted to. It has already been pointed out that the tremolo type of figure has a peculiar continuity (p. 279). This continuity of the tremolo is often used to keep a melody alive and vivid. The early compositions written for the harpsichord and clavichord have a variety of tremolo figures in the melody; trills, turns, runs and all sorts of embellishments really superfluous to the true melodic notes. The same thing appears in the arrangements made for the street-piano whose melodies must carry long distances and compete with street noises.

It is a familiar fact that the melody is the most important voice, and that the time-keeping part comes next in importance. Usually the time-keeping part is the lowest part, probably because the lower tones stimulate larger, slower muscle-sets. The other parts for the average hearer or player often degenerate into mere harmonic factors, chords, and do not figure as true independent melodies unless some peculiarity brings them out.

As to the actual muscle sets at work in a musical rhythm it is hard to say. In many cases it seems doubtful if the muscles of the performer which actually produce the tones can be the source of the rhythm. So many non-rhythmical movements enter in — movements up and down the keyboard of piano and organ, from lever to lever in wind instruments, movements of the bow and up and down finger-board in violin. And the same muscles have to execute a large number of extraneous movements, such as turning pages, pressing pedals, pulling stops, etc. When a melody at the piano is played by the thumbs alternated as chance permits, it seems probable that the real rhythmic process of the melody is independent of the hands and that they are controlled by it. An independent process of takt-keeping is very common.

The phrases and periods of modern music are very clearly defined. Although elaborate phrases both with and without rhymes had been familiar in poetry, rhythmic periods with articulate parts were not developed in the history of musical form until harmony had reached a stage when the harmonic period was possible.[1] The takt of the older contrapuntal music was used simply to regulate the ensemble. This shows how intimately the larger grouping of musical rhythms has been associated with harmonic and melodic relations. In modern music the phrasing is remarkably organic. In many periods the end of each section, and the end of each phrase, have definite harmonic relations which correlate them, and make it possible for the listener to say just what point has been reached in the process. The cadence at the end of the period is always an unequivocal indication of rhythmical finality. In the melody, also, the use of imitation in the figures and intervals, and the

[1] Parry, C. H. H., *Evolution of the Art of Music*, New York, 1896, p. 109.

repetition of sections or phrases serve to articulate the rhythm.

The dynamic forms of the larger unities are not so definite, and there is often a choice of forms; but here, also, there is frequently a tendency to accentuate the phrasing.

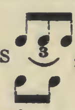
It is characteristic of rhythm that many of the apparent irregularities of music should in reality be normal elements in the grouping. At the close of a phrase or period long notes are frequently shortened, and short notes are lengthened. In records of hymns, played by excellent musicians, made by Sears[1] such variations show very clearly; some of the deviations from the mathematical intervals are very wide. At the end of the period, the general movement of the phrase comes to a close, and the tensions of at least a part of the processes die out before they begin again. The pause at the end of the phrase is for this purpose, and its length does not depend on the indicated interval, but on the requirement of the particular type of movement. In some cases (as in Lux Benigna, Dykes) one of the rhythms continues through the pause, and the finality process of the phrase must accomodate itself to the intervals of that rhythm. In mechanical devices, and in badly done ensemble playing, the takt is kept rigidly through every pause. Such a process does not contribute to the regularity of the rhythm, and in some cases may seriously disturb it.

Aside from such 'irregularities' which the rhythms require, there are various minor variations for the purpose of expression. These are not essential to the rhythm, neither do they disturb it in any way. Expression which disturbs the rhythm is false expression. A gradual increase or decrease of tempo (accelerando and ritardando) are very common. The hold on a single note usually marks a phrasing pause. A slight variation of the intervals of one rhythm without affecting the intervals of the secondary rhythm is a very effective device, and is the true rubato, rather than the utter failure to keep the time which goes by that name.

In considering the possible effect of 'content' on rhythmization, the song often shows a wide variety of rhythms for ap-

[1] Sears, C. H., 'A Contribution to the Psychology of Rhythm,' *Am. J. Psy.*, 13, p. 28.

proximately the same content. Heine's 'Du Bist Wie Eine Blume' has its own effective rhythm, but it is not followed by the musical settings of either Rubenstein, Liszt, Schumann, or Cantor. Their rhythms are all different, yet all appropriate, and in a sense expressive.

The laboratory analysis of musical rhythm should be much more easy than that of verse rhythm. The pitch relations are definitely determined, and the manner of tone production permits a fairly accurate determination of intensities in at least one instrument of wide application, the piano. Registering apparatus for duration and intensity has been applied to some extent. Sears[1] has published some satisfactory records of the temporal values of several compositions played at the organ and Binet and Courtier have studied[2] the temporal and intensity values of several musical forms played on the piano, but the intensity of each separate note was not recorded, and the record is not satisfactory for the detailed study of rhythm. An efficient recording apparatus for the piano presents no serious difficulties. A careful study of the details of the variety of musical figures, of the many combinations such as , of syncopation, of ritardando and accelerando, and of the temporal and dynamic conditions of phrasing should yield valuable results for the general theory of rhythm. Changes of tempo offer an excellent method of studying the emotional effects of rhythm.

V. The Central Processes of Rhythm and of Discrete Succession in General.

The well-known action theory of Münsterberg sets forth the principle that the outgoing nerve currents have as much to do with the determination of a content of consciousness as the incoming currents. Vividness is the important attribute of the sensation which is determined by the outgoing currents.[1] In view of the action-theory, there are two ways in which one may

[1] *Loc. cit.*

[2] Binet, A., et Courtier, 'Récherches graphiques sur la musique,' *L'An. Psy.*, '96, pp. 201 ff.

[3] Münsterberg, H., *Grundzüge der Psychologie*, '00, Leipzig, S. 525 ff.

picture the central processes involved in a motor theory of rhythm. The first way and the one founded on the ordinary view that contents in consciousness are the result of afferent currents, is to consider a rhythm as a series of kinæsthetic sensations from the various joints and muscles involved in the rhythmic movement. During the relaxation-phase there are sensations of changes of tension with the slow movement of the joint; during the contraction-phase there is first a rapid movement of the joint, followed by sudden, intense sensations of tension at the end of the beat-stroke. The grouping and connections are due to the connection of these kinæsthetic sensations, and the striking differences in vividness are due to the changes of intensity in the kinæsthetic sensations, especially of the muscular strains. Such an interpretation is very natural, but it involves difficulties when the rhythmization of other than kinæsthetic sensations is considered. Instead of referring the changes in a uniform sound series which we voluntarily rhythmize to a superposed kinæsthetic series, we speak of the sounds themselves as changed; they are now nearer together, 'grouped,' and their relative vividness (usually referred to a supposed change in intensity) has become different. All these properties of the rhythmized sound we refer to the sound sensations and to them alone. Indeed, there are some who would deny after a careful introspection that movements have anything to do, directly or indirectly, with the rhythmic perception of the sound series.

Instead of giving to kinæsthetic sensations the anomalous capacity of changing the vividness of other sensations of an entirely different order, it is far better to make the motor theory of rhythm an application of the action theory of Münsterberg to this particular field. Then the changes in vividness of the sounds are not due to the afferent kinæsthetic sensations of the accompanying movements, but to the efferent currents which produce the movement. The innervation of the musculature in the perception of a series of sounds leads to the observed changes in vividness. An essential factor then in a rhythmic series of kinæsthetic sensations is the series of efferent processes producing the rhythmic movement.

A rhythm is a voluntary temporal succession with voluntary changes of vividness of sensory material. It should therefore present an excellent field for detailed and controlled experimentation on the 'time sense' and the process to which changes in vividness are due. There are already a number of observed facts of considerable theoretical interest. A rhythm as the one accurate measurement of time; the phenomena of temporal displacement; the phenomena of subjective accentuation; the fact that at high rates of succession sensations appear in consciousness simultaneously; the fact that the limit of rapidity in hearing a series and of producing one by movements is approximately the same; the fact that in rapid succession certain sensations at regular intervals may not appear in consciousness though their intensity is the same as that of intervening sensations which are perceived; radical changes of a rhythm due to mere changes of tempo; the phenomena of temporal displacement in nearly simultaneous sensations of different orders; all these should throw some light on the problem of the perception of succession.

1. *Central Processes During the Movement-Cycle.*

During the back-stroke both the positive and negative muscle-sets are contracting under the continuous guidance of the kinæsthetic sensations which pour in from the moving parts. It is a 'regulated' movement; as Rieger puts it, one that does not happen more rapidly than the nervous changes in the muscles can take place. All of which means that during the back-stroke there is a constant regulating flow of nerve currents from the movement-localization (sensory) centers to the efferent motor centers. And on that constant inflow of sensory currents, and outflow of regulating currents the sense of continuous change in the temporal localization series depends. But when the beat-stroke begins all this is changed. No longer are both sets of muscles contracted, and no longer are they continually inhibited and stimulated by currents from the localization center. Instead, the negative set relaxes suddenly and the positive set contracts, then without any regulation from the localization center, the positive set relaxes, the limb is carried along by inertia for some

sig. and then, still without any regulation, the negative set contracts automatically at the end of the beat-stroke. Then the tension of the negative set decreases, the positive set contracts slightly and the usual type of movement takes place during the the next back-stroke. It is evident that at the beginning of the beat-stroke the moter centers suddenly cut loose from the regulating localization centers and perform their function quite automatically until the close of the beat-stroke; the explosive reaction seems to be performed at the cue of an image of the movement, nothing more. The localization centers do not come into touch again with the movement until the end of the beat-stroke. And all the sensations occurring during the movement seem to be telescoped as it were into the end of the movement. At that point they all seem to get their place in the localization series.

The vividness of the sensations occuring during the movement-cycle is independent of their objective intensity and depends on their position in the movement-cycle.

In imagining the central processes which the facts require, one must reason from crude mechanical analogies. But at bottom, the central processes must be mechanical and more elaborate and complicated than any possible construction. One of the striking facts about the rapid type of movement of the beat-stroke is that its rapidity is not increased by practice. There are variations among different parts of the body and among different subjects, but even the long training of piano playing covering an extended period of growth cannot alter the rapidity of the movement.[1]

As previously noted, the maximal number of movements per second possible to a limb depends on the duration of the beat-stroke. This definite and unchangeable interval of the beat-stroke points to a fixed type of mechanical arrangement between the motor centers governing the positive and negative muscle-sets. The coördination of muscles into negative and positive sets is very easily changed and readjusted, but while it persists, it represents some fixed type of mechanical connection. We may imagine that the centers of the positive and

[1] Reif, *loc. cit.*

negative muscle-sets are arranged tandem, so that the efferent current must first cause the positive center to react, before it can overflow into the negative center; the time required to effect this serial stimulation is the duration of the beat-stroke. Or, since the action is really a double one in each center, because the muscle-set first contracts and then relaxes, we may imagine that the action is controlled by a mechanism vaguely like the cut-off mechanism of a reciprocating steam-engine.

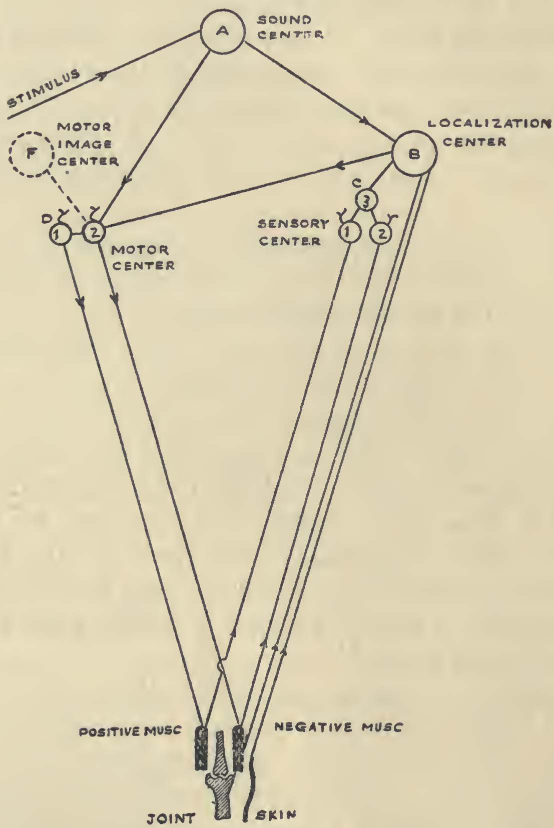


FIG. II.

As for the changing tensions between two contracting muscles which are essential to the sense of continuous change, one may suppose that the 'center' in which this sense of change is produced is always affected by a difference of tension, and has the

same connection with the afferent centers for tension sensations from the two muscle-sets as has the galvanometer in the Wheatstone bridge. The whole process may be roughly figured as below, Fig. 11.

A represents the sound center from which a direct reflex runs to B (1 and 2), the motor center. B is the localization center for the series of sensations during the movement-cycle; it receives stimulations from the joints and skin, and difference stimulations from C, 3 which stands in bridge relation to 1 and 2 of C which receive the afferent tension stimulations from the muscles. In the process of rhythmization a sound stimulus passes to B, where it gets its position in the time series, and at the same time the outflow from A and B to D determine its vividness. During the time of equilibrium, just before each beat-stroke, the steady outflow from B would give the sound a a slight vividness, and the inflow of currents to B would make the sound continuous with the preceding sensations of the back-stroke. If the center B were connected with other localization centers, in which changes were taking place (as with a secondary rhythm), then the sound during the 'pause' would find a place in the other series. But at the beginning of the beat-stroke the connection between the localization center B and the motor center D suddenly ceases. This inhibition is brought about perhaps by a difference stimulation-process (since the muscles are now in different phases, one contracting and the other relaxing), perhaps by sudden increase of tension in D due to an inflow from the motor image center E which makes it impossible for B to discharge into D, and thereby preventing the incoming sensation from receiving any vividness till the tension of D decreases. At the close of the stroke, the inhibition of the flow from the localization center B to motor center D is removed, and the normal condition of B is restored. And now all the stimulations which are still alive in C and A, or are on the way flow at once into B, and get their position in the time series en masse. Thus all the sensations which occur during the beat-stroke and persist to the end of the beat-stroke are referred to that point.

2. *Discrete Succession.*

The fact that the maximal rate at which we can perceive sensations as clearly successive is approximately the rate at which a series of beats can be produced points to a close relation between the movement of the ballistic type and the perception of discrete succession. In the case of sound it is clear that the minimal interval between two sounds which are perceived as successive, is *not* determined by the necessary duration of a stimulus to the ear. A sound may be heard and its pitch determined when two vibrations are given. This gives a varying minimal length for perceived tone.[1] From C_1 to g^4 this means durations varying from 60.6–0.63 sig. With this in mind Abraham and Schaefer[2] made a series of experiments to determine how rapidly two tones might follow each other. Using a variety of musical intervals they found that the minimal interval between the tones is approximately the same for all octaves, and that the observed variations follow no regular curve. In all tones above B_1 the minimal interval in a trill or tremolo is longer than the time for the recognition of pitch. In octaves c^2, c^3, c^4, it is from eight to fifty times as long as the duration necessary for determining the pitch. In musical figures, the interval was rather longer than for trills and tremolos. The length of the interval for trills or tremolos was 28–42 sig.; for musical figures involving at least three pitches the minimal interval approximated 100 sig. This is also the range of the duration of a beat-stroke in a rhythm, and as Reif points out, the minimal interval in produced rhythms.[3] Wundt gives the minimal interval as determined for various senses as from 16–47 sig.[4] Exner and Weyer found an anomalous value for hearing of 2–5 sig., which is probably not the interval of a true succession, but of some sensation quality. A continuation of a series at such an interval would, of course, produce a tone. Hylan gives the minimal interval for hearing for eight subjects

[1] Abraham, O., u. Brühl, L. J., *Zeitsch. f. Psy. u. Phys. d. S. org.*, '98, 18, S. 177 ff.

[2] Abraham, O., Schaefer, K. L., 'Über die Maximale Geschwindigkeit von Tonfolgen,' *Ztschr. f. Psy. u. Phys. d. S. org.*, '99, 20, S. 403.

[3] Reif, *loc. cit.*

[4] Wundt, W., *Grundzüge*, 5te Aufl., Leipzig, '03, 3, S. 46.

as ranging from 26–86 sig., average, 47 sig.[1] The maximal interval for the appearance of a musical tone is variously given as 30–60 sig.

When a sound stimulus flows in over the nerves, it is not heard, *i. e.*, does not attain vividness and appear in consciousness, unless it obtains the reflex discharge shown in Fig. 10 by the connection between A and D. This reflex is the essential element in the 'attention' to the sound and is necessary to any motor theory of rhythmization of a sound series. This reflex is probably a contraction of both the positive and negative sets of the musculature (position and extent unknown); in the case of 'starting' at a sound, it even seems to be of the ballistic type. If the sound continues, the contraction and consequent tension between the muscle-sets continues unless inhibited by other processes (as when one fails to hear a familiar continuous sound). The reaction seems to be stronger at first than shortly after, and this gives a certain pulse to the earlier part of the sound. If the sound is short, this sudden contraction which gives a vividness of the pulsation type is the entire reaction, and the positive and negative muscles relax. The sudden contraction is the condition on which the pulse of the new sound-beat depends, it marks the new thing, and without it there can be no discrete succession. If the sound is repeated at short intervals, the motor reaction is thrown into the cyclic form. The most rapid form of cyclic movement in which the sudden contractions of the type necessary to discrete succession can be produced, is that of the double ballistic stroke as described by Richer and Rieger. This is the form which the motor reaction must take in perceiving a discrete succession at a maximal rate.

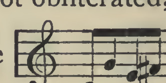
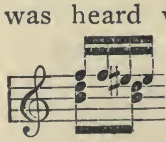
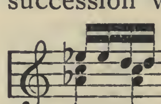
Some of the phenomena observed by Abraham and Schaefer and for which they offer no explanation are of the greatest interest in the light of this hypothesis.[2] In judging musical figures containing three or more different pitches in succession, they found that at the most rapid rate the tones were heard as a chord; no succession was detected, although the stimulation

[1] Hylan, J. P., 'The Distribution of the Attention,' PSY. REV., '03, 10, p. 402.

[2] Abraham and Schaefer, *loc. cit.*

was manifestly a matter of succession. In the resultant chord, the order was in no wise determined by the actual order in which the notes were given; moreover the notes were frequently mistaken. What the subjects did hear were the ordinary combinations of such tones into chords, and unusual notes were frequently replaced by more usual ones. Hylan has observed precisely the same thing in the case of visual sensations. Hylan's[1] explanation is that 'somewhere on the route of transmission the visual impulses from different points of the retina traverse paths sufficiently common to cause the impulses received first to overcome the inertia, thus allowing the later impulses to overtake the first and so to reach the center of consciousness nearly or quite simultaneously with them.'

But it seems to the writer more simple to refer the phenomenon to the fact that succession is the result of a distinct motor process requiring more time than the interval of the succession in question affords.

As the rate of the tone succession was gradually lessened, Otto and Schaefer found that a succession might be heard in which the material present was modified. Intermediate tones were often omitted outright, though their intensity equalled that of the other tones. In several cases *alternate beats of the same tone were omitted;* this happened with all the observers. This means that the motor process which orders these beats into succession and gives them the vividness by which they appear in consciousness may react so as to suppress or ignore every other beat in a uniform series. Frequently tones were not obliterated, but carried over to the next beat; *e. g.*, the figure  was heard when the interval of succession was 28 sig. as , and at 59 sigmas as  (at 95 sig. the figure was correctly perceived). This shows that the motor process has reduplicated upper notes and misplaced lower notes, carrying them over an entire beat. The motor process involved in the perception of such a series must consist of

[1] Hylan, *loc. cit.*, p. 403.

ballistic strokes in opposite directions (or of ballistic strokes in the same direction, if more than two members are involved). If the movement of the motor process of the lower notes was running but half as fast as the objective beating, every other note coincided with each pulse (end of a beat-stroke) of the movement, and each intervening note occurred *during* a ballistic stroke. If the note occurs at the pulse of the movement, it will receive the vividness of a beat and appear in consciousness. If a note occurs *during a ballistic stroke, it will either be lost, or it will be referred to the pulse at the end of the stroke.* If the activity caused by the stimulation persists until the end of the stroke, the sound will be perceived at that point; if the activity of the stimulus dies out, then the sound is lost. In the cases just mentioned, where a tone appearing but once in the figure was not perceived, the stimulus must have died out before the end of the movement. In the cases where the sound was misplaced one beat the stimulus must have persisted. In the case of alternate sounds of the same pitch which seemed to be lost, it may be that they simply fused with the same sound occurring at the pulse of the movement.

Precisely the same phenomena in the case of sensations of sight connected with movements have been observed by Dodge [1] and carefully studied by Holt.[2] The movements of the eye in question are extremely rapid. Dodge and Cline give the duration of eye movements of 5–40 deg. as 34.5–100 sig.,[3] which agrees very well with the duration of the beat-stroke in the rhythm-movement. It has been demonstrated that a sensation of sight occurring during such a movement of the eye is not located during the movement, but at the point where the movement stops, just as the sensations of sound in a rhythmic beat-stroke are referred to the end of the stroke. By skilful management Holt reduced the intensity of the stimulation during the movement until the stimulation died out before the end of the movement was reached, and proved that such a

[1] Dodge, R., 'Eye Movements,' PSY. REV., '00, 7, p. 454 ff.

[2] Holt, E. B., 'Eye Movement and Central Anesthesia,' *Harvard Psy. Stud.*, I., '03, p. 1 ff.

[3] Dodge, R., and Cline, T. S., 'Eye Movements,' PSY. REV., '01, 8, p. 155.

stimulation, though readily perceived by the eye at rest, *is invisible when occurring during the movement.*[2] Holt explains this 'central anesthesia' by assuming localization centers in connection with the movements of the eye (corresponding to *B* and *C* of Fig. 11, above) which discharge to the motor center of the muscles (corresponding to *D*). The vividness of the incoming visual sensation is produced by the discharge from the color centers (corresponding to *A*) to the localization center (B and *C*) and thence to the motor centers (*D*). "During visual anesthesia muscular sensations of present movement are streaming to consciousness, to form the basis of the new post-motum localization." "And these would have to go * * * to the localization, or eye-muscle sensation centers." "One may well suppose that the incoming centers are thus raised to such a tension that for the moment no discharge can take place thither from other parts of the brain, among which are the centers for color sensations" (*i. e.*, tension in *B* so high that *A* could not discharge into *B*). It is to be noted that this phenomenon in the case of visual sensations involves a temporal displacement exactly similar to that of the rhythmic process, and like the rhythmic displacement, it occurs in connection with the rapid movement, and only after it is well under way. Holt's explanation would be equally applicable to the cause of rhythmic displacement. But there is this objection. The cause of the high tension in *B* (Fig. 11) is supposed to be the rapidly incoming currents from muscles and surfaces. In the eye, the epithelial surfaces may be supposed to furnish a large volume of such incoming sensations. But in the rhythmic movement the one point where a large number of intense sensations flow in is not during the movement, but at the close of the movement when sensations of strain, and perhaps of impact and sound, come in in large volume. It seems more probable that the disturbance of the temporal series is due to the peculiar innervation of the muscle-sets in the rapid movement, followed by the vividness due to the discharge at the close of the movement. Whatever the explanation, it should apply to rhythmic temporal displacement, to the theory of localization and motor anesthesia in the

[1] *Loc. cit.*, p. 44.

eye, to the maximal rate of succession, and to the omission of notes in rapid series as observed by Abraham and Schaefer.

It is remarkable that while the eye-ball is an extremely mobile organ, and many of its movements are expressly described as rhythmical[1] its movements do not seem to lead to a perception of rhythm. Movements of the head and eyelids may be rhythmized, but the fact that the eye-movements have an essential part to play in another type of localization series seems to prevent the eye from figuring as an organ of rhythm.

In discussing the vexed question of simultaneity it is important to distinguish two distinct types; first, the type of simultaneity observed by Hylan in a very rapid succession of visual sensations (interval less than 20 sig.) and of sound sensations as observed by Abraham and Schaefer, on the one hand; second, the type of simultaneity which involves two coinciding rhythmic pulses, or two sensations located in two different temporal series, as in the well-known 'complication' experiments. In the case of simultaneity due to very rapid succession, the sensations simply enter consciousness en masse, and since they get but one motor reaction, they are not separated and have but one place in the temporal series. It probably happens that the succession in some cases gives the mass of sensations a peculiar sensory *quality*. Exner's anomalous interval with spark may have been of that type. In the experiments of Abraham and Schaefer it was often possible to determine that the chord was not a mere chord before any temporal succession could be detected. In the case of true temporal simultaneity, one temporal series is superposed on another temporal series, and a given sensation in the one series coincides with a given sensation in the other series. It is known that such coincidence is not determined by the objective relations, and that the 'attention' to the one or the other of the processes alters the reaction.[2] This is probably due to the directness with which the sensations are connected with the motor apparatus, and the possibility of the one occurring during the beat-stroke time-gap of the other.

That such speculations do not represent the actual facts in the central process is more than probable. But however crude

[1] *Cf.* Dodge, Psy. Rev., '00, 7, p. 457 as to eye-movements in reading.

[2] Wundt, *Grundzüge*, 5te Aufl., 3, S. 86.

or inexact such constructions may be, the investigation of certain aspects of rhythm, especially of the events during the movement-cycle and of the phenomena of temporal displacement and subjective accentuation, should be an excellent means of approach to the theoretical problems of succession and relative vividness. A rhythm can be controlled in detail, and its motor character adapts it to experimentation.

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The Psychological Review.

STUDIES FROM THE BRYN MAWR COLLEGE PSYCHOLOGICAL LABORATORY.

An Experiment in Learning to Make Hand Movements.[1]

BY PROFESSOR JAMES H. LEUBA AND MISS WINIFRED HYDE,

Bryn Mawr College.

Between the first and the thirtieth of March, 1904, forty-two students averaging twenty-one years of age, nearly all undergraduate, undertook to learn to read and write German script under the conditions mentioned below.

The task of these forty-two students was a double one: to put English prose into German script and to write in English script English prose which they had before them in German script. Thus, only German script, and not the German language, is involved in this experiment. They were asked to do as much work as possible and at the same time to try and write with constant distinctness.

The subjects were divided in two groups. The twenty-six subjects of Group A began with the writing. Of these, twelve, after having gained a considerable writing ability, applied themselves to the reading. The sixteen subjects of Group B began with the *reading* of the German script. Of these, thirteen passed later on to the task undertaken first by the other group, *i. e.*, the writing in German script.

[1] This investigation was planned by the undersigned and Miss Hyde, at the time a graduate student at Bryn Mawr College. The data were gathered by the latter who intended to make the experiment her own. But, after having plotted the individual curves, lack of time compelled her to leave the work unfinished. In the spring of 1905 the undersigned took it up where she had left off and wrote the present paper.—J. H. Leuba.

Every period of work was of twenty minutes' duration. Of Group A, six subjects met twice a day, after breakfast at 8:40 A. M. and after luncheon at 1:40 P. M.; seven met once a day at 1:40 P. M., except No. 7, who worked at 8:40 A. M.; six met every other day at the same afternoon hour, and the others every three days, also in the afternoon. Of Group B, five met twice daily at the hours first stated: five once a day at 1:40 P. M., three met every other day at the same hour and the others every third day, also at the same afternoon hour.

In a few cases students prevented from meeting at their accustomed time changed their hour.

A time-keeper called time at intervals of five minutes during the twenty-minute periods and the subjects made a mark in their records, thus enabling us to follow the work done during each five minutes. When, as it happened a number of times, the time-keeper forgot to call time, he then gave the signal after six minutes, and reduced the following period to four minutes. The inequalities thus introduced in the records was corrected by taking as a basis for calculation the number of letters written or read during the whole period. A few, very few, other irregularities were corrected in the same, or in a similar way. They could not have had any noticeable influence on the conclusions here set forth.

Eleven of our subjects (Nos. 1, 7, 12, 13, 14, 24, 26, 27, 31, 33, 39) had already had some practice in German script. But it was, in every case, several years before and amounted to extremely little, except in the case of No. 7, who still remembered a few letters. The records of these persons were, however, left out in the consideration of every point in which previous practice might have modified the results.

Contents:

1. General comments on the individual curves and analysis of the psycho-physiological processes with which this experiment is concerned.

2. The influence on writing of reading practice and vice-versa.

3. Fatigue.

4. The fluctuations in effectiveness: a comparison of the amount of work done during the successive five minutes of the twenty-minute periods.

I. *General Comments on the Individual Curves and Analysis of the Psycho-physiological Processes with which this Experiment is Concerned.*

In the eight annexed curves (Charts 1 and 2), given as samples of individual curves, each segment corresponds to one period of work. The figures to the left indicate the number of letters written or read. The height of each square is equivalent to twenty-five letters.

The irregularities observed by previous investigators who dealt with the acquisition of motor skill[1] are in evidence in our curves: instead of ascending steadily they frequently show drops without any periodicity.

The causes of these irregularities are no doubt many. The fact that instead of decreasing with habituation the irregularities continue approximately as marked and, in the case of the writing curves, become even greater, might point to weariness as an important factor. At first, and for a number of periods, novelty helps the student to keep to the highest point of efficiency. But, when the task has become monotonous, there are moments of relaxation. Slight fatigue, no more overcome by interest, brings about a flagging of attention. The reader should, however, observe that since each element of the curves represents an absolute and not a proportional gain, the irregularities of the curves would increase even though the irregularities of the relative gains should not: a loss of 5 per cent., for instance, would be expressed by a much smaller drop at the beginning of the curve when only 200 or 300 letters are written in twenty minutes than later on when a thousand letters are written in the same time.

The rise of the curves, very rapid at first, soon decreases considerably, but no plateau appears within the limits of our curves. What would have happened, if the practice had been continued

[1] See, for instance, Swift's 'Studies in the Psycho-physiology of Learning,' *Amer. Jr. of Psy.*, 1903, Vol. XIV., p. 211.

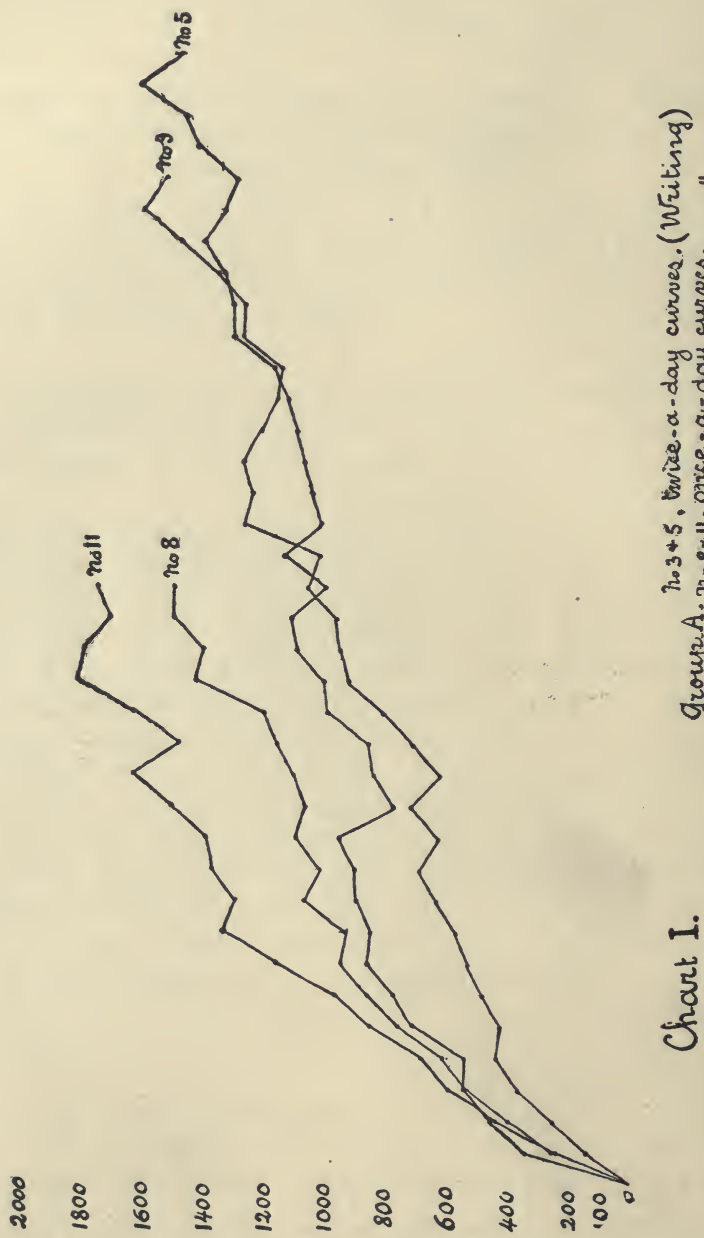
Chart I.
Group A. No 3 + 5, twice-a-day curves. (Writing)
No 8 + 11, once-a-day curves. "
No 5
No 3
No 11
No 8
2000
1800
1600
1400
1200
1000
800
600
400
200
100
0

CHART I.

much longer, can only be inferred. The twice-a-day curves stop at a maximal rate of from 1,400 to 1,800 German letters a period, whereas a test of the speed of these subjects in writing in English gave an average of 2,277. The subjects were therefore far from having reached the rate of writing possible to them in English. The flattening of the curves, already well marked

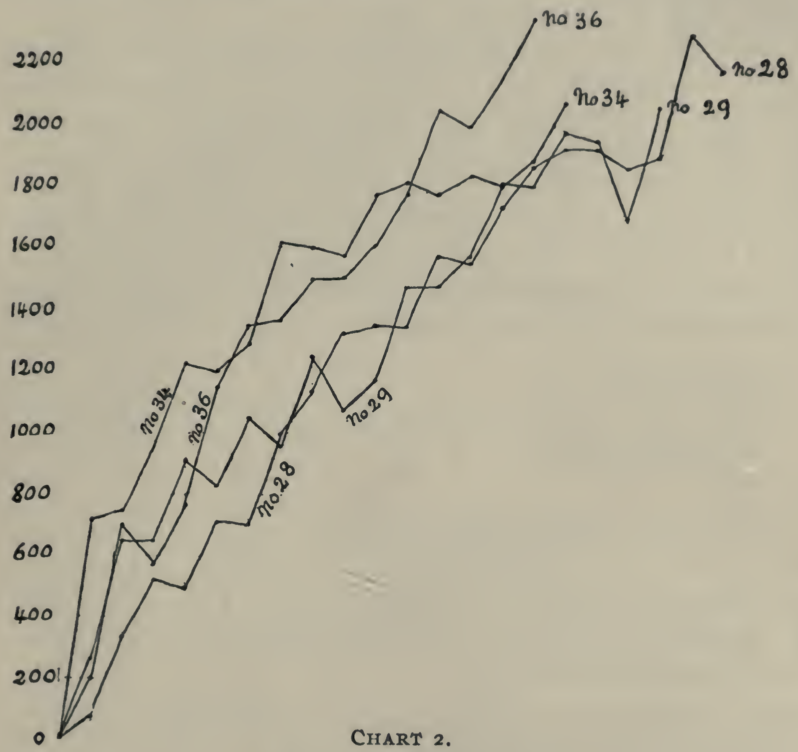

CHART 2.

Group B. no 28 & 29, twice-a-day curves. (Reading.)
no 34 & 36, once-a-day curves "

in the longer of the writing curves, would no doubt have increased with the continuation of the practice. Would a characteristic rise have appeared later on? There are no reasons known to us for an affirmative answer; for even though the several psycho-physiological processes involved in the learning of German script under the conditions of this experiment, should

appear in a definite order of succession, they do not each reach complete maturity before the next one becomes possible. On the contrary, several of them grow together, and before they have been fully perfected the next ones are already in operation.

But what are the several processes involved in learning to put in German script English prose ?[1]

Leaving out of consideration what belongs to the volitional order, and speaking as if perceptions and representations ordinarily led of themselves to purposive movements, we have first the establishment of associations between, on the one hand, the dynamic system formed by the visual perceptions of discreet English letters and the speech-motor and auditory representations they evoke, and, on the other, the visual perceptions of the corresponding German letters seen by the subject in the alphabet with which he is provided :

Visual perception of English letters plus speech-motor and auditory representations.	Associated with visual perceptions of German letters.

But, as the German letters are seen, they are also inwardly named and heard (in English), and in addition, the intent to write them determines motor discharges which reproduce more or less perfectly the letters. The preceding schema must therefore be completed thus :

Visual perception of English letters plus speech-motor and auditory respresentations.	Associated with visual perceptions of German letters, associated themselves with	Speech-motor and auditory representations of the letters plus the motor and other sensible antecedents of the motor discharge.

The repetition of this process under the pressure of the desire to write rapidly brings about a double short circuiting. (1) The student need no longer refer to the alphabet; the *perception* of the German letters becomes unnecessary. It is replaced by representation of them. (2) At the same time the

[1] Comp. 'The Acquisition of a Hierarchy of Habits,' by Wm. L. Bryan and Noble Harter, PSY. REV., Vol. VI., 1899.

speech-motor and the auditory representative elements which follow upon the sight of the German letters and precede the writing of them, drop out. This means the formation of a direct connection between the visual image of the German letters and the cue to the motor discharge. We come thus to the second phase of the first stage of the process:

Visual perception of English letters plus speech-motor and auditory representations.	=	Visual representation of German letters.	=	The motor and other sensible antecedents of the motor discharge.

With sufficient practice this second phase passes into a third one, in which the ordinary speech-motor and auditory associates of the perception of the English letters and, in addition, the sensible antecedents of the motor discharge fall out of consciousness. At this culminating point of the first stage the writing follows immediately — automatically — upon the representation of the German letters, itself preceded by nothing more than the perception of the English letters. The automatization may even go so far as to eliminate the image of the German letters; so that, finally, they may be written directly on the perception of the English letters without any conscious intermediary.

But when this automatic process has become possible, frequent lapses are observed which bring back the subject for a while to an earlier, less mechanical, phase.

So much for the first stage. So far the individual letters have been the objects of attention. The moment comes when groups of letters — syllables or words — are substituted for the letters. Instead, for instance, of starting successively from the perception of the three letters composing the article *the*, the *word* appears in the mind as a whole, as a unit. Its auditory and visual apprehension and the sensational outcome of the innervation which would utter it and write it, take the place of the three successive and completely different mental contents represented respectively by the three letters *t*, *h*, *e*.

The substitution of the word-apprehension for that of the component letters is, it hardly need be said, of the greatest practical importance. It is also one of the most interesting facts encountered in the psychology of writing and of reading.

How is this substitution made possible?

While it is true that the *writer* at first considers the letters separately, he does not fail to also apprehend the word which they form. (At the beginning this may be true of the small words only.) When he wishes to write, for instance, the word *man*, before he pays attention to the letters he apprehends the word and pronounces it mentally. Again, upon the completion of the last letter, the word as a whole reappears in consciousness. The schema given above is therefore incomplete; it should begin and end with the apprehension of the word itself. As the *writers* become familiar with the forms of the German letters and cease to be dependent upon the sight of them, the word-apprehension becomes an increasingly considerable part of the consciousness accompanying the writing and, when the final phase of the first stage has been reached, it is found that the word-consciousness has been, in the case of many small and oft-recurring words, substituted for the separate letters as cue to the writing.

The substitution of the word for the letters is thus seen to take place in the same way and according to the same principles as the earlier substitution of the image of the German letters for the perception of them.

The subjects forming Group B began, as already said, with the reading of English prose put in German script. They were, however, not only to read the German script, but to write it out in English. These *readers* passed from the visual perception of the German letters (devoid at first of any meaning) to the writing of the understood English letters. In every respect other than this different starting point and what evidently follows from it, the psychological processes are the same in the case of Group B as in that of Group A.

The fact which we have affirmed in saying that each particular element of the learning process was not perfected before some other, higher, ones appeared, is established by introspective evidence and confirmed, it seems, by the absence of plateaux in the curves. It was with our subjects as with Swift.[1] "All factors of

[1] 'The Acquisition of Skill in Typewriting,' Edgar James Swift, Psy. Bull., I., No. 9, Aug. 15, 1904, p. 302.

the perfected process have clearly been present almost from the start and the only justification for characterizing any particular stage by one element rather than the other is the prominence of the one or the other factor." Small words are quite early dealt with as units. This taken together with the unsteadiness of the recently formed syntheses would lead one to expect an ascending curve without genuine plateaux but with many irregularities.

II. *The Influence on Writing of Reading Practice and Vice Versa.*

The figures given in the following table are averages of the work done by all the subjects except those who had some acquaintance with German script at the beginning of the experiment.

GROUP A. (Writing preceded reading.)

First writing period	260	letters.
Second " "	414	"
Last " "	1,319	"
First reading period (after an average of 19 writing periods)	1,147	"

GROUP B. (Reading preceded writing.)

First reading period	346	letters.
Second " "	660	"
Last " "	2,070	"
First writing period (after an average of 15 reading periods)	470	"

It may appear surprising that after having acquired a degree of skill in reading German script measured by 2,070 letters (written in English script), the subjects should have been able at first to turn into German script only 470 English letters, *i. e.*, *hardly more than was achieved, already in the second period, by those who had not had previous reading training.*

What the 'readers' had learned was to recognize *visually* the German letters and to express that recognition in the familiar movements tracing the English letters. The figures given above show plainly that visual recognition — a visual acquaintance sufficient for identification — is a very small part of what has to be acquired in order to be able to reproduce with the hand identified forms. Whoever has tried to reproduce from memory a map, every part of which could have been easily recognized, has felt the difference there is between rec-

ognizing a visual form and reproducing it. But a more thorough comparison of the reading and of the writing processes is needed here.

When reading, the subject, starting from the German script, endeavors to pass to the writing of the English forms already known to him. In doing this he passes through an intermediary step; the naming of the letters. When writing, he starts from the English script and tries to write the corresponding German forms. Here also he passes through the intermediary step of naming the letters. In both cases associations have to be established between the English and the German forms, but whereas in reading the movement of attention is from the second to the first, in writing it is from the first to the second.

Now, it has long been known that the ability to recall a series of ideas following each other in a particular sequence does not carry with it the ability to repeat it in another order. Ebbinghaus and others have, however, shown that something is gained by learning in one order a succession of speech movements which one wishes to reproduce in another. The difference between the number of German letters written in the first period by those who began with the writing (260) and the number of German letters written in the first period by those who had had the reading practice (470) measures this gain in the present instance.

If we consider now the advantage derived from a writing knowledge when learning to read (Group A), we find that whereas, on an average, 1,319 letters were written in German during the last writing period, only 1,147 German letters were read and written in English during the first succeeding reading period, a loss of 172 letters! Whence this surprising loss? Does not the learning to write involve all the elements which enter into the learning to read and, in addition, the acquisition of new hand movements? Yes, the elements are the same, but not the direction of the associations. In the one case, as we have just said, the visual perception of the German letters is associated with the English forms, in the other it is the reverse.

The considerable gains shown in the first reading period following the writing practice over the first reading period not

preceded by writing — a gain of 801 letters — is to be ascribed to three causes: (1) the familiarity gained, while writing, with the appearance of the German letters; (2) the associations established, although they link A with B and not B with A; (3) the undeniable, although slight, reading practice gained by the writers in the very process of writing. For, as one writes, and especially as one pens the last letter of a syllable and of a word, it is almost impossible not to read what has just been written.

III. *Fatigue.*

A. Comparison of the amount of work done in two daily practices, in one daily practice and in one practice every other day, each of twenty-minute duration.[1]

Chart 3 shows that the gains in writing-skill for equal amounts of time was least for those who worked twice a day and greatest for those who practiced once a day or every other day. Practice every third day gave gains half way between the extremes.[2]

Curve A is an average twice-a-day writing curve; B an average once-a-day curve; C an average every-other-day curve and D an average every-third-day curve. A and B are each made out of 5 individual curves, while C and D are made out of 4 and 3 individual curves respectively.

AVERAGE NUMBER OF LETTERS WRITTEN IN TWENTY MINUTES.

	Twice-a-Day. Subjects.	Once-a-Day.	Every-Other-Day.	Every-Third-Day.
After 5 practices.	625	825	780	750
After 10 practices.	865	1,115	1,175	985
After 16 practices.	1,015	1,540	——	——

[1] Previous experimenters on the general question of the acquisition of motor skill either did not regard the question of rest-intervals or dealt with it so differently from us that their results are not comparable with ours. Amberg, Rivers and Kraepelin, and Lindley have studied the influence, upon adding and memorizing, of periods of rest varying from 5 minutes to an hour in duration. Swift has recorded in several places the effect on motor efficiency of a practice-interruption of several weeks and even of several months.

[2] In tracing these and also the other curves reproduced here the records of the subjects who had had previous practice were omitted.

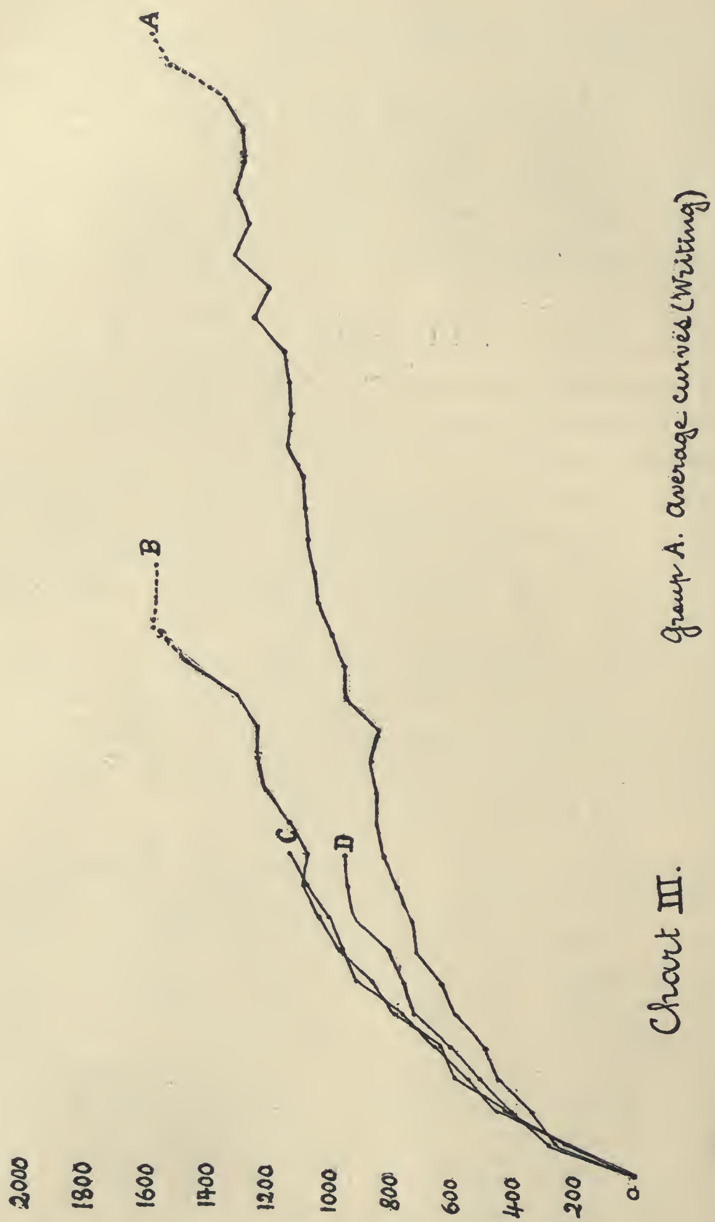
A
B
C
D
2000
1800
1600
1400
1200
1000
800
600
400
200
0
Chart III.
Group A. Average curves (Writing)

CHART 3.

The once-a-day and the every-other-day curves fall upon each other. It is a matter for regret that these two curves and the every-third-day curve could not have been continued as long as the twice-a-day curve. The figures on p. 361 will help in establishing the correspondence existing between these curves.

Two conclusions seem, therefore, warranted: (1) Under the conditions of this experiment the fatigue incurred in learning to write new script is too great to give the best results when there are two twenty-minute practices each day. (2) An interval of three days between the practices is too long to give the most economical results.

If the fatigue detrimental to the twice-a-day writers arose mainly from the efforts involved in learning to make *new* hand movements and not from the other factors, the reading curves should not show the differences we have just noticed in the writing curves. Unfortunately our data concerning reading are not full enough to admit of safe conclusions.

The average once-a-day and every-other-day reading curves of Group A are made up of too few individual curves and are too short to have any value. In Chart 4, *C* (the every-other-day and the every-third-day curve) is an average of only two curves and stops after 8 practices. It cannot, therefore, be of any service in this connection. The other curves of this chart do not differ enough one from the other to permit of discrimination. *A*, the twice-a-day curve, is an average made out of 5 individual curves, and *B* is a once-a-day curve made out of 4 individual curves. Curve *A* of Chart 5 would seem to indicate that two reading periods of 20 minutes are somewhat better than one a day *for a person who has already become acquainted with the visual appearance of the German script*, even though, as was the case, the German script read had to be written in English. But here again the data are hardly sufficient to carry conviction; *B*, the once-a-day-curve, represents only thirty-one practices divided between five subjects. We had better, therefore, be guided by the comparisons established in Section B.

B. Comparison of the morning with the afternoon gains.

The first column of the following table gives in per cents. the average gains made in the afternoon when compared with the

morning's practices. The second column gives the gain of the morning over the preceding afternoon's periods.

AVERAGE INDIVIDUAL GAINS IN PER CENTS.

Subjects.	Writing.		Reading.	
Group A.	After Morning Interval.	After Night Interval.	After Morning Interval.	After Night Interval.
1	2.5	4.7	—0.3	6.7
2	2.7	3.6		
3	2.6	8.3	—2.5	15.7
4	2.1	6.1	—2.3	13
5	0.86	6.8	—2.6	8.1
6	3.5	4.2	2.5	3.9
Group B.	Reading.		Writing.	
29	2.3	15	4.9	1.1
30	2.06	9.7	3.9	7.8
31	15.5	16.9	4.6	3.2

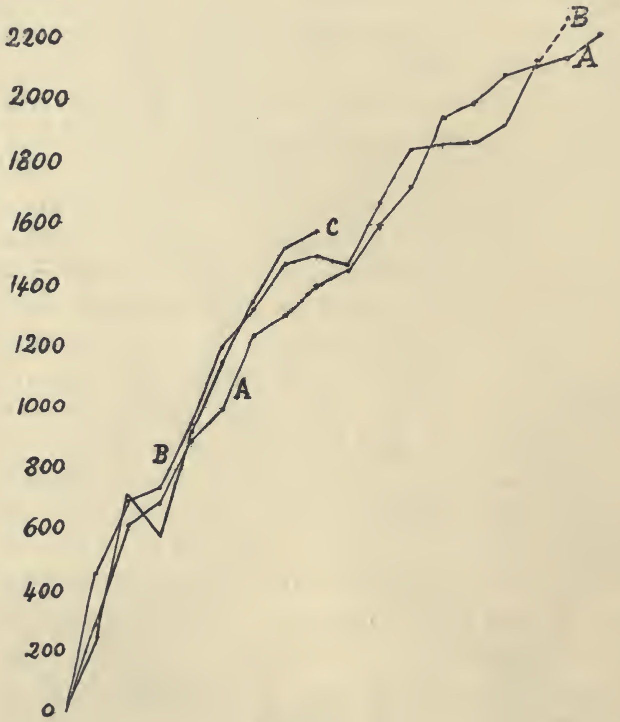

CHART 4.

Group B. Average curves (Reading)

An interval of about four hours, filled with varied intellectual work and much taking of lecture notes, had elapsed between the morning and the afternoon exercise, while twenty hours, including the night's rest, separated the morning from the preceding afternoon's practice. In every case, except in the writing of 29 and of 31, a gain, usually a considerable gain, is

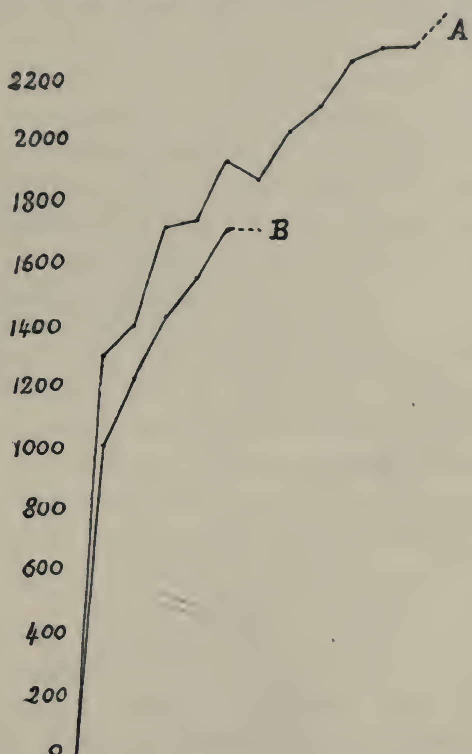

CHART 5.

Group A. Average curves (Readiug)

shown in the morning and only a smaller one and, at times, a positive loss, appears in the afternoon. In the reading of four out of six subjects of Group A there is an absolute loss in the afternoon.

What portion of the fatigue so clearly shown in the afternoon's work is to be charged to the morning's practice and what to the intellectual work and to the note-taking of the

students during the four intervening hours? This query is answered convincingly by the fact that the practice of the subjects who show the most rapid progress — once-a-day subjects — was done at 1.40 P. M., except in the case of 7, who did her work at 8.40 A. M. But the curve of 7 was left out of the average curve because she had had some previous practice. Had the single daily practice taken place in the morning instead of in the afternoon, the superiority of the once-a-day curve would have been still greater.

The morning practice and not the students' ordinary morning's work is, it appears, chiefly responsible for the decline in the work done in the afternoon by twice-a-day subjects.

It is to be observed that the afternoon's loss is even more evident in the reading than in the writing. It may well be assumed that it would not have been so if, when reading, the subjects had not had to write in English what they read in German. This seems indicated by a comparison of the reading figures in Group A with those of Group B. In Group A the reading came at the end of the writing practice and the number of letters written in English script was therefore from the start throughout much greater than for the subjects of Group B, who began with reading. This would lead one to expect a greater loss in the afternoon reading of Group A than in that of Group B. That is precisely what the figures show.

The conclusions drawn above are thus confirmed by a comparison of the morning with the afternoon's work and we may consider it demonstrated that for persons of the age of our subjects, intent upon doing the most possible in twenty minutes' time, one practice a day yields the best results in learning to write German script or any other similar script.

Whether shortening the practice period to fifteen or to ten minutes would lead to another result remains an open question.

The bearing of this conclusion upon the length and frequency of the writing lessons of school children is indefinite since they take their work much more leisurely than our students did, and since, being much younger, they tire sooner. For the rest, we do not know that anywhere school children have two writing lessons a day.

Some additional information on fatigue will find place at the end of the next and final section of this paper.

IV. *Comparison of the Amount of Work Done During the Successive Five-minutes of the Twenty-minute Periods.*

The marked oscillations in effectiveness observable in a period of work of twenty minutes' duration are functions of many independent variables, fatigue, ennui, excitability, 'entrainement,' etc.[1] These factors themselves vary, of course, from day to day with the general condition of the subject which, in its turn, depends upon a complicated network of causes.

Out of eleven subjects who practiced twice a day, only two show in their averages an uninterrupted progress from the beginning to the end of the practice: No. 6 with the writing averages, 10,803, 10,816, 10,991, 11,062, and No. 31 with the reading averages, 6,700, 6,861, 6,868, 7,132 and the writing averages, 7,223, 7,367, 7,400, 7,530. The other members of this class show a drop in the second, or in the third and sometimes in both the second and the third five-minute. The best marked instance of this form of periodicity is found in the reading of No. 27 whose averages are 7,903, 7,635, 7,759, 8,466. In all but three averages of these eleven subjects, the last figure is the highest. In these three exceptional cases, the amount of work done during the first five-minute is never equalled in the rest of the practice. In general, one may say that there is a decrease in efficiency in the second five-minute, a recovery in the third five-minute and an additional increase in the last.

But these figures cannot be taken as a satisfactory basis for the measurement of fatigue, since there are several other factors which affect one's productivity. Ennui, excitability, 'entrainement,' each play a rôle in determining the periodicity of these figures. So do also the gains in skill. These gains would, of course, tend to increase the figures as one passes from the first to the last five minutes of each period and therefore would partly mask the effect of fatigue. A more or less definite idea of the extent to which this takes place may be gained from the con-

[1] See, concerning these factors, the excellent work of Lindley, 'Ueber Arbeit u. Ruhe,' *Psy. Arb.*, III., Heft. 3, pp. 482–534.

sideration of the records of Nos. 1 and 3. The total figures for the successive five minutes are:

No. 3	7,493	7,228	7,538	7,309
No. 1	11,901	11,645	11,616	11,670

Comparing the first with the last column, we find a loss for No. 3 of 184 letters and for No. 1 of 231 letters. And yet these two persons gained greatly in skill during the course of the experiment, as is shown by the number of letters they wrote during the last twenty-minute period when compared with what they accomplished during the first:

No. 3	first period	150	last period	1,570
No. 1	" "	450	" "	1,710

A probably valid reconciliation of these apparently contradictory figures lies in the admission that as the twenty-minute proceed, increasing fatigue conceals the increasing skill. The gains, therefore, do not appear in a comparison of the first with the last part of any one period, or of all of them together, as is done in the above figures, but in the comparison of any part of a period with the corresponding part of any subsequent period, or in the comparison of the twenty-minute periods considered in their entirety.

The case of the subjects who practiced once a day only, confirms the foregoing observation on fatigue. Out of six subjects who wrote once a day four show an uninterrupted gain in the successive five minutes; and out of five subjects who read once a day two fall in the same class, whereas, we found only two similar cases among the eleven twice-a-day subjects.

The gain factor is eliminated from the following figures which represent the number of letters written in English from *English* prose. Gains due to increased skill could hardly be expected in the course of a twenty-minute exercise in English writing, since it is constantly practiced by students. The first line gives the averages from the 27 of our subjects who were submitted to the test of writing in English from English copy as fast as they could for twenty minutes. The two other lines are two individual records.

Average	622	613	628	625
Individual	883	762	794	740
"	528	500	565	565

In these English tests the first figure is frequently the highest; only in a small number of cases is the last the highest, although there is no doubt that the subjects made special efforts (excitability, *entrainement*) to surpass themselves in the last part of the twenty minute period. The third period gives the highest average.

The rough general interpretation one may give of these more or less regular fluctuations is as follows: After the first effort has spent itself, there comes a relaxation due to fatigue and weariness. It is overcome by the desire to do one's best when the signal for the beginning of the third five-minute warns the subject of the approaching end and it would be overcome still more completely during the last five-minute if fatigue had not meanwhile become too great. It should not be overlooked that the announcement of the time every five minutes changed to some extent the natural periodicity.[1]

[1] The MS. of this article was received June 28, '05. — Ed.

A STUDY OF THE MOTOR PHENOMENA IN CHOREA.[1]

BY DR. G. M. PARKER,
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It is the purpose of the paper to study the movements observed in chorea. This is done with a complete awareness of the narrow field implied, yet with the intention of developing through this limited objective certain definite though different views of the pathology or psychopathology of chorea. The study is not one involving the basic pathological causes, but it is rather an analysis of its predominant manifestations with the aim of more clearly defining the existent physiological conditions through a psychological and biological interpretation of the motor phenomena.

It will be shown that the choreic movements are not incoordinate in a biological sense; that rather they are reversions to type, which, at an earlier point in the development of the individual and the race, have been highly fit.

It will next be shown that in choreic cases these movements may artificially be produced; that their production will invariably be seen to rest upon an attempted functioning of the higher, more complex motor systems; that these choreic movements, capable of approximate measurement, become the more exaggerated, *pari passu*, with the increasing complexity of movement attempted.

Finally, that this implicit relationship develops the conception of a motor hierarchy in which the more complex movements are compounded from the more simple; that, in chorea, there is an inhibition of the functioning of the higher motor systems with the resultant of movements called by Janet 'derivative,'

[1] The contents of this paper is based upon cases studied in the Neurological Department of Vanderbilt Clinic under Dr. Starr, the experimental work having been done in the department of Physiology by permission of Dr. Curtis and Dr. Lee.

movements primal, simple, fixed, but which, early in the developmental scale, have been the component parts out of which the higher types have been evolved and into which the higher is again resolved under certain pathological conditions represented in chorea.

The necessities of this demonstration although few are absolute. There must first be gathered observations of choreic cases as they spontaneously present; the varying and divergent motor elements must be shown as related by common characters and possible of subsumption under wider generalizations. Following this, coördinate with it, must succeed observations derived from cases artificially varied by experimentation, tending thus to throw into relief with clearer outlines the foregoing elements and their relations. Third, these spontaneous and artificially produced phenomena, these inductions must be shown as necessitating a single definite hypothesis.

I. The movements of the choreic child are said to be incoordinate, quantitatively in excess, qualitatively unfit. The fundamental point is the unfitness. In a chronological sequence the early choreic movements fall under a type which is designated as coaptive; the child is to be seen rubbing his fingers one against the other, passing them over the palm of his hand, snapping them together, touching with them different objects in what seems to be a useless, idle manner; his feet, his toes can be noted in analogous attitudes and movements; his tongue, his lips are in constant contact, the tongue being passed over the lips in and out of the mouth, the lips being pressed or rubbed together in a varied and bizarre series of movements. In the constant maintenance and variation of sensory contact of different approximating sensory surfaces is to be found the necessity for the term 'coaptive.'

Even in these coaptive movements the tendency is toward grossness rather than fineness; as the symptoms progress the former is more regularly observed. The movements are quantitatively in excess; they are gross, but they are especially gross because they involve the larger muscular groups and joints in the progress of the disease. The sudden swing of the arm or leg, the bending at the waist, the inclination of the head, the

sudden transposition of the entire body are essentially large movements; large in type as well as large quantitatively. The small fine type movement does not present in chorea save at the terminals.

These movements show further a tendency toward bilateral action. This is not literal, but relative; that the left leg swings violently does not imply an equally wide excursion of the right, but the right leg relatively often would move though not so violently; in the arm it is more constant, the excursion of the minor arm not being an exact verisimilitude of the major, but approximating to it. Facially it is even more noticeable, the grimace being either simultaneously or successively bilateral.

The choreic movements are not single; they tend to repeat themselves or variants of themselves rapidly and persistently, especially if non-interference be advised. The coaptive movements repeat time after time; they change much less abruptly than is apparent, for between two apparently divergent movements there have been interpolated several repetitive series. The gross movements have often been observed as occurring and recurring in series: the child stamps not once, but several times; her body bends again and again; she inclines her head rapidly; there is repetition in all the movements and at all stages of the disease.

Beyond these several purely motor phenomena are to be found a series which, in comparison, are more complex, more organized, and apparently more coördinate because more fit. They differ from the preceding because the end seems partially purposive, though none the less quantitatively the reaction be excessive. The child is seen to leap, to bound, to jump over obstacles, run with enormous strides, or with an uneven speed, varying from immobility to rapid transit. Within the confines of the room, his acts are doubly exaggerated; he leaps over chairs, slams the door with violence, often moves the furniture in the room, particularly those pieces the disturbance of which produces the most noise, throws light objects about, nothing delighting him more than thus pushing or throwing; he strikes suddenly, not viciously or continuously; the mother says he is rough, loud and disturbing; that he cannot remain quiet; if a

girl, that she has become a hoyden; that she no longer plays with the girls, but seeks the rougher sports of the boy. Yet in these the intelligent parent never sees exuberance. Although the acts are purposive, they are not such as to fall within the accustomed habitude of the child; they are not 'fit' for him.

To determine the signification of this unfitness necessitates a return to the question of coördination in this specific type of cases. Broadly speaking coördination connotes a reaction, an adjustment, an adaptation between two factors; namely, the individual at that particular developmental point and that individual's environment, which means, of course, nothing more than the most fit adjustment between the internal and the external.

Coördination is the most fit adjustment. If we adhere rigidly to this, there is nought else for it except that choreic movements are not coördinate. Yet we are not absolutely transfixed in this dilemma. We have said that coördination is the most fit adjustment with due regard to the developmental stage of the individual, the internal factor in its relation to the environment. A reaction which is fit for a two-year-old child is coördinate, yet this same reaction would be unfit, hence incoördinate for one six years old.

Is there possible then in chorea a technical evasion of the law? Are there here motor phenomena belonging to an earlier phylogenetic stage, with the consequent characteristics and their unfitness or incoördination for the present stage?

The coaptive type of movement is one of earliest life; they are the avenues of knowledge as to the material Ego; they are the sensory stuff from which the earliest nucleus of the personality is formed. The babe is seen slowly, discursively passing his fingers over the hand, slowly moving his toes, protruding his tongue, approximating his legs, rubbing his hands over the body in such a manner as to combine the greatest touch area with the greatest kinæsthetic excursion. These movements are present for many months; their appearance is relatively constant in point of development; their transition into reactions to the environment is gradual. They, as coaptive movements, as the first contribution to the ego, are primal, necessary, fit.

Though they be awkward, crude, irregular, superficially non-purposive, yet they are fit — for a definite period of development.

Grossness in movement is displayed early in the child's life in its varied reactions to the external environment. The coaptive movements of the hand while small are never fine; they affect the entire finger, not the terminal phalanx; the excursions are as wide as the function of the joint will permit. It is continually noted that small movements are difficult; that motion involving the large joints presents far more frequently than that involving the smaller; that this relative predominance persists to a much later stage of the child's life than is supposed; that much of the old pedagogical knowledge and kindergarten work has been revised upon these and analogous lines; that finally, the gross movement is that movement which to the individual at an early developmental stage has been and is most fit.

The bilateral movement is never outgrown; in childhood it is everywhere to be seen. The reason for its being is most apparent; the reason for its especial fitness at early life is purely biological; it is fit; hence it presents.

The repetitive movement, the circular reaction of Baldwin, is most important to the individual in his early life. It is the repetitive movement, the incessant repeating which allows of those sudden variations, not *de novo* changes, from which in turn spring the new movement. It is seen in every act; practically no act is single in the child or babe; he repeats and repeats; imitates his own act until there is a chance variation, when, in a second this variation is repeated in a series as was the first. Its importance is enormous; its fixation as a biological stage is absolute; it is seen in all grades of mind and until a relatively late stage of development. It seems non-purposive, yet it is fit to the highest degree — during a certain developmental epoch.

Those movements which have been described as more complex, more purposive, yet mal-adapted may well fall within that type which Groos in his *Play of Man* describes as the play movement. Groos in this work from which much has been

drawn, develops the biological necessity of play for man. In the earlier bizarre jumping, leaping, throwing, thrusting, rough play he sees that which was of service to the race at an early epoch and now which tends to occur at constant periods of life. If unrestrained, untrained, the ordinary child is usually something of a savage; he does not walk, he bounds; his speech is loud and insistent; he delights to throw rocks, to strike, to push, to thrust. To feel the activity of life, sensory and motor, is to him the summum bonum. Yet all this is fit for a certain developmental epoch. Their advent is not viewed as pathological by rational parents, be the manifestations ever so disagreeable, disturbing.

Resolved thus into their elements the choreic movements appear differently. They are incoördinate for the particular developmental point at which they appear in the specific case. They are no longer fit reactions. But, biologically viewed, these same movements have at an earlier phylogenetic point been highly fit, highly adaptive, hence under the broad acceptation of the term highly coördinate.

II. Beyond, however, this interrelation, this interconnection with earlier phylogenetic series we have not progressed. We have but shown that choreic movements may be resolved into elementary movements, which, in the history of the individual should find themselves at an earlier stage. How these reversionary types present is next to be considered. It can readily be said that they present because they are possible alone, else the higher, more fit, would appear, as it is always the more fit adaption which persists if conditions permit.

Can we show that these lower organized movements are more attainable or, expressed negatively, can we show that the higher combinations are less possible to attain and upon attempted functioning are productive of widely diffusive motor phenomena, which phenomena are those of low and simple organization. If this can be shown, namely that the choreic movements, movements proven to be of low type, reversionary, are produced because of the impossibility of higher functioning, the functioning of more complex systems, are in other words, quoting Janet, 'motor derivatives,' diffuse motor phenomena, then the train of causative sequences will be further advanced.

The main point here, namely, this tendency towards motor diffusion or motor derivatives upon attempted functioning of higher systems requires notice, as upon this line the experments were based. A movement can never be absolutely simple ; there is always more or less diffusion into other channels. Movements which are simple, which are thoroughly organized, which are fixed, are those in which the motor portion of the arc is most definite ; there is here less diffusion into collateral channels. Movements, however, which are complex, which are highly rather than thoroughly organized, which are not fixed but are rather continuously displaying variation are those movements in which the motor arc is indefinite, not fixed, hence here we have a tendency towards diffusion into collateral channels or systems, and especially into those systems representing that earlier, simpler, more fixed combination from which the more complex has been builded. In chorea the extension of this physiological tendency beyond normal physiological limits into the realm of the pathological affords the *raison d'etre* of the experimental work.

The line of experimentation can herewith be seen. The patient must be made to perform a series of movements, graduated from simplicity and low organization to complexity, all of which movements must fall within the line marked out for the individual by the study of the normal, within general developmental boundary lines. During this experimentation, it must be so arranged that measurements can be made of certain of the most simple and fundamental movements, which movements in their rise and fall may indicate definitely the degree of motor diffusion.

Taking this measureable motor diffusion, then, as an index, we may in turn measure the degree of functioning possible in the different motor coördinations performed by the subject under experimentation. Let the functioning necessary to a certain motor performance become more difficult, less possible to attain by a particular individual, then the curve of his motor derivative will display this in its increased irregularity, its grossness and other characteristics later to be noted. Measure this curve against that of the normal child at the same age and developmental

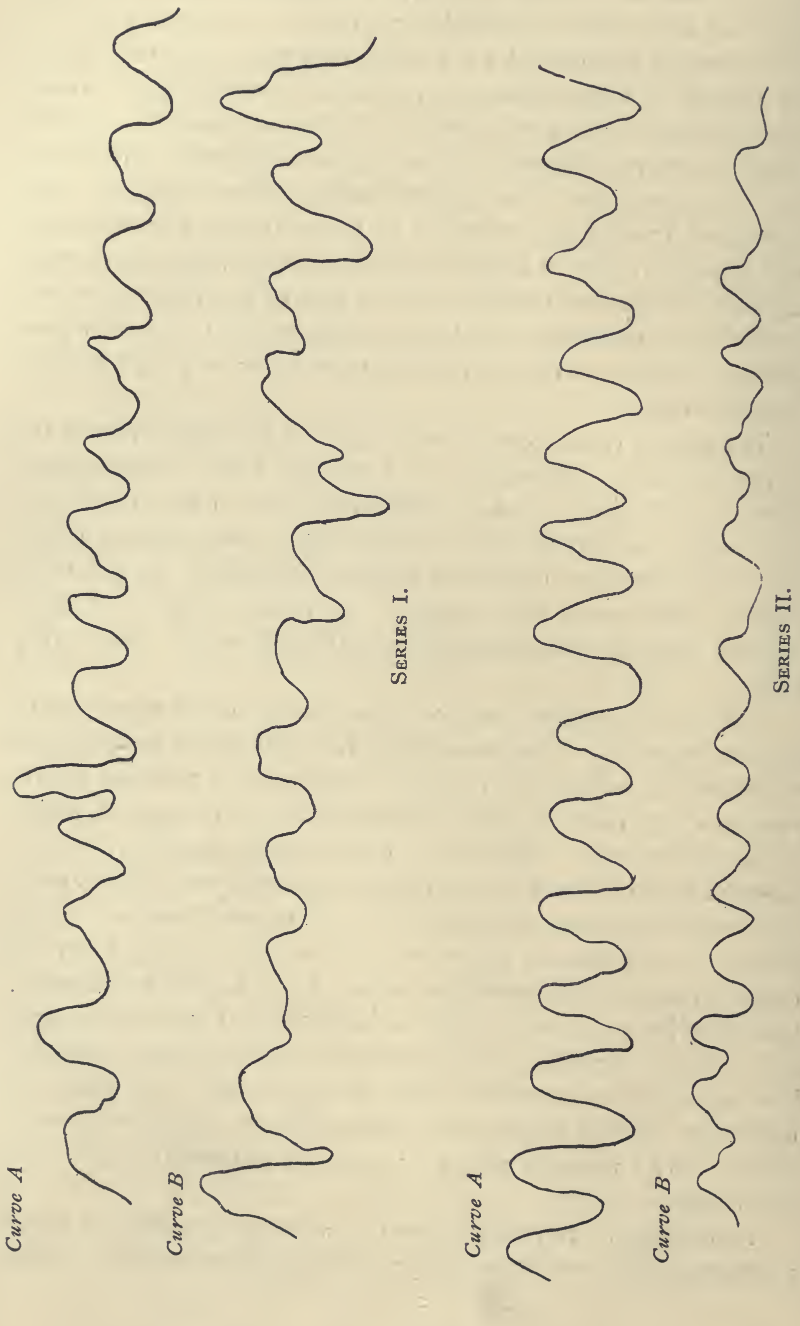
point of the subject, and there will be obtained the proof of the abnormal character of the subject's curve. Further, with this determination one should have a fair measure of the psychological change in his functioning, as well as the meaning in terms of psychology, of the presentation of the involuntary, incoördinate phenomena observed in chorea and analyzed in part one.

For the purpose of the measurements above referred to, the respiratory tracing was selected as presenting the readiest and most accurate curve of a motility phenomenon, simple, primitive and fixed in the developmental scale, and as one which presents invariably in any motor diffusion or presentation of lower phenomena. Hence it can be called a constant and as such a fairly accurate index.

The age of cases experimented upon ranged from five to ten or twelve years. Mentally they averaged well; no deficiency noted in any form; all were clinic cases, but cases from families of the respectable and self-respecting poor among whom the children are not carelessly raised. The work was done in a quiet undisturbed environment. A certain degree of fear, initially present was permitted to subside before beginning work.

With the respiratory belt over the thorax, cases were started upon movements. The movements were subject to considerable modification during the course of the work, it proving necessary in every case to lower the grade of the movement below the mean average. The series were finally ranged in a manner such as to begin with the simple coaptive type, thence passing through several gradations to the circular reactive type, thence to the definitely imitative type seen in copying from the flat in a series of increasingly complex lines, angles and figures, finally arriving at what might be called the constructive type, that of the drawing and formation of idealized figures. Through this necessarily crude series, it will be seen that movements of low organization gradually give place to those of higher. The tracings will be presented pursuant to this general schema.

In tracing *A*, Series I., there is seen the respiratory curve accompanying the horizontal type of coaptive movement. This


Curve A
Curve B
Series I.
Curve A
Curve B
Series II.

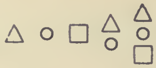
horizontal movement is one of the simplest; it contains the rudiments of the first movements of acquisition and defence. Expressed graphically it is thus ☰. It forms the basis of the earliest variants. In this movement the babe is earliest seen to display its motility, and from this, viewed as a circular reaction type, are derived numerous other movements. There is noted here but relatively slight disturbances of the curve.

Immediately below this curve, however, is placed another accompanying the vertical type of movement, tracing *B*. This movement, vertical in form, performed in a line parallel to the axis of the body, becomes useful and fit at a much later stage than does the horizontal. These curves, obtained from a single source under similar conditions, are widely different. Curve *B* shows the motor diffusion as represented by sudden, deep prolonged inspiration with exaggerated intervals between inspiration and expiration, all so charted as to appear typical only of an agitated respiration. Certain accompanying motor phenomena common to this and other diffuse phenomena to follow will later be considered.

In a way the motor phenomena thus far expressed and performed by the patient might well fall within the type of the circular reaction, *i. e.*, one movement grossly reduplicating itself, notwithstanding the fact that these two movements, horizontal and vertical, were so different in general results. In order to express more clearly this circular reaction with a movement commonly easy, the general form of recurring circle was used, thus combined with a horizontal excursion spatially.

The Curve *a*, Series II., accompanying this, is herewith shown. It is seen to correspond in general regularity of excursion to that of the horizontal in Curve *a*, Series I. It thus appears that the circular reaction presents the characteristics of a simple, lowly organized motor phenomenon.

For contrast there is juxtaposed (Curve *B*), Series II., a curve taken from the same patient under similar conditions, the motor performance alone being varied. This consisted in drawing in the flat: *i. e.*, in drawing of two dimensions, length and breadth but no depth. This drawing was imitative copying from model given, the model being simple, thus:

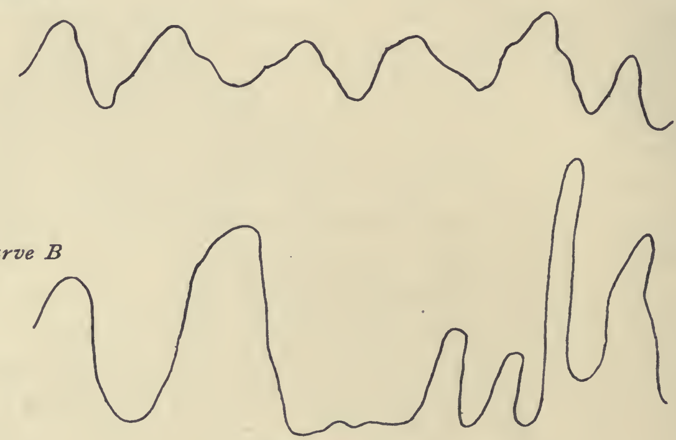


Simple though it be, the necessity for coördinate movement, the motor arc of which must proceed through definite and highly organized systems, was productive of the motor diffusion below graphically displayed.

A third series (Curve 3, *a* and *b*) from another case is next presented, in which the motor performance is precisely similar, but in which the motor diffusion seen in the copying from the

Curve A

Curve B



SERIES III.

flat is far more pronounced. We have here a further corroboration as to the general viewpoint. This series was performed by a child two years younger in age and educational development than the preceding. Drawing in the flat to this child normally implied a more difficult coördination, yet normally falling well within her developmental range. It represented for her a higher relative degree of functioning than for individual preceding; it was hence more difficult, less possible, with motor diffusion more pronounced and wider.

Series IV., Curve (*a* and *b*) further illustrates this point. Here Curve *A* is one accompanying a drawing on the flat as in the preceding. In Curve *B*, however, the patient has not drawn

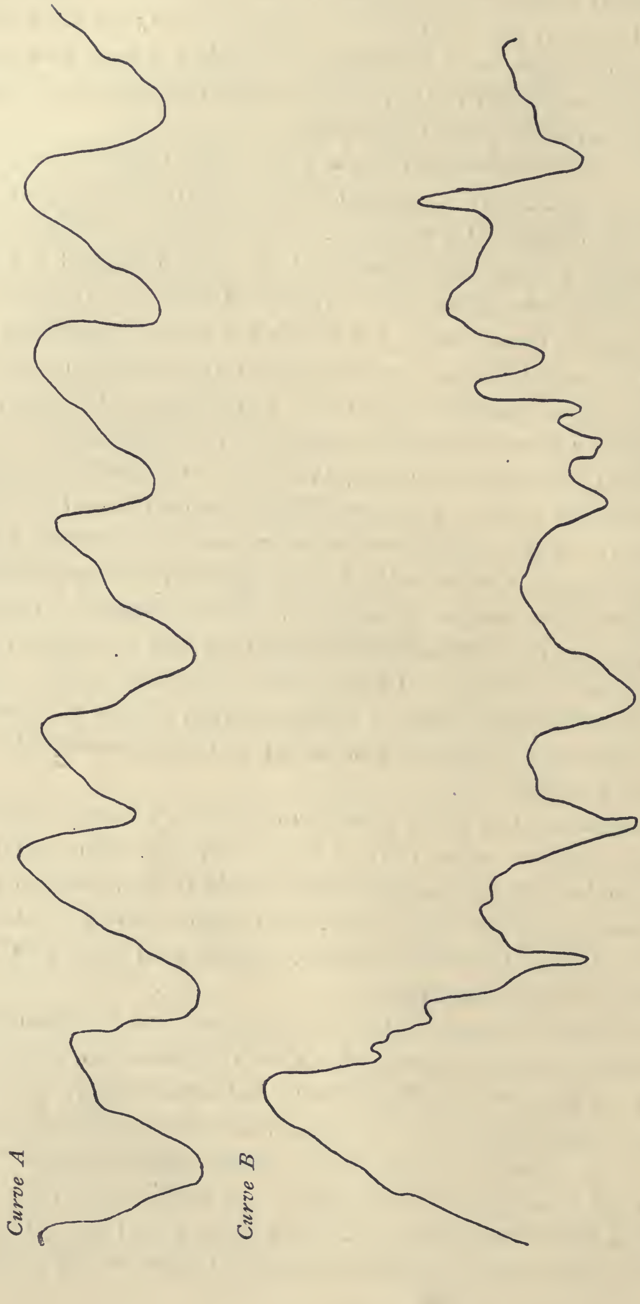
from a copy, imitatively, but has rather carried out the lines of a familiar object, not then in his range of vision; he has ideally constructed a copy, a performance involving wider and more complex systems than the former, hence phylogenetically more recently fixed than those preceding.

It is as well to be noted that, in series with which we have already had to do, Curve *A*, Series IV., is much more regular than Curve *B* in Series III., to which it corresponds. This follows from the age of patient in Series IV., to whom the flat drawing fell far within the ordinary developmental range. He had been so long trained as to render the motor arc fixed, coördinate and fit. To this same child, as we have seen, the idealized drawing, although again falling within his developmental range, yet necessitated a functioning of systems in a way different. Here in the latter there were no sensory fields, no persistent centripetal sensations with a correspondingly limited motor arc as in the former, the drawing from the flat; there was merely representative sensory content with its less defined systems through which the motor impulse proceeded. The absence of constant sensory corrective, the indefinite limitation and the greater variability of the motor arc, all these make for a more highly organized degree of functioning, a more complex degree of functioning. The motor diffusion, the motor derivatives hereby result as shown in Curve *B*.

In no one of the cases does the respiratory curve represent any more than a mere part of the motor diffusion which was evidenced but not grapically reproducible in the sudden gross movements of body, legs, arms, the coaptive series seen in the movements of the hands, the feet, tongue and lips, and other numerous motor derivatives.

In no case, wherein the curve displayed the marked motor diffusion, was the experimental act well performed; it was performed not only with difficulty but most imperfectly.

The result of the experimentation may be briefly subsumed; that in the choreic subject, motor functions, normally falling within the developmental range of the child, were here, first, produced imperfectly; second, their production gave rise to a series of phenomena characterized by a wide motor diffusion



Series IV.

into systems whose motor equivalents consisted of primitive simple, early placed reactions, part of which, graphically measureable, is here presented, part of which, impossible to measure, is descriptively added; that the motor functions productive of these phenomena are regularly and invariably those in which the motor portion of the arc is most highly organized, with larger collateral channels, greater variability, hence expressing a finer, higher adjustment to the environment; that, contrariwise, the phylogenetically simple fixed movements are produced readily, perfectly, with no accompanying motor derivatives.

Correlating now parts 1 and 2, it has been seen that, in chorea, those movements spontaneously present, which, from a viewpoint of biology, are to be considered as simple, primitive, if of low organization; which, furthermore, have subserved a useful part in the history of the individual, have been fit at a certain developmental stage.

Experimentally we have produced phenomena similar to the spontaneous choreic movement by the imposition of definite conditions upon the patient. These conditions have been comprehended within the performance of certain complex acts. These acts, falling well within the normal range, have been seen as less possible to the choreic, less perfectly performed, and invariably productive of the phenomena above remarked.

III. Thus far the phenomena have been described, the conditions rendered exact, but explanation beyond this has not been given. In chorea, why does the production or attempt at production of motor acts of high order produce or tend to produce these motor phenomena of low order which present as the most striking symptom of chorea? By the use of the terms 'high' and 'low' order, there has been implied a gradation of motor function.

This gradation psychology terms the motor hierarchy; its ramifications, its limits have been mapped out and determined from many viewpoints. Its content is that which has been epitomized in the choreic case; a series of motor reactions ranging from the most simple to the most complex. Its organization is a series of interrelations. The hierarchy is not additive, *i. e.*, it has not been formed by the mere addition of one

movement to an earlier movement; it is rather a true organized hierarchy in that each movement higher has developed from a variant of an earlier and lower type. It will be recalled that the circular reaction of Baldwin was of that type from which the variant springs, which more and more frequently recurs, then gradually assumes the circular type itself in the perfectioning, and in its turn affords the 'motor platform' from which another variant arises. With an increasing necessity of more fit reactions to the environment, these variants are more determined towards complexity, until the motor functions of high rank are complex and coördinate to an extent of which we have no fair appreciation. This we fail to see, because, in their performance, it is but the end term of the reaction which we perceive and of which we are conscious. Yet, because the component elements of these highly coördinate and complex movements lie below the threshold, because they are the penumbra of the focal motor phenomenon, they cannot be denied. They are demonstrable beyond question in the normal, as myographic tracings have repeatedly shown the presence of the earlier elementary types in the functioning of complex movements.

Now let this complex act be unattainable or with difficulty attainable, then will follow that which lies at the basis of Janet's law of psychological derivatives. This law is as follows: When a force, originally intended to be used for the production of a certain phenomenon, remains unused (for this specific purpose) because this phenomenon has become unattainable, then there are produced derivative phenomena, *i. e.*, the force expends itself in producing other phenomena incoördinate and useless.

We, however, go further in showing that the derivative phenomena are elementary, primitive types of movement which, in the motor hierarchy, have been gradually integrated into higher and more complex systems, have existed beneath the threshold of consciousness and in their spontaneous presentation here as choreic movements, apparently incoördinate, unfit, involuntary, are to be considered as the elements out of which the higher movements have evolved.

We are, thus, not forced to the conception of the overflow

of neuron energy into other and diverse channels, with all the indefiniteness which this conception of 'overflow' implies. It is needful only to view the motor neurons as functioning in systems, which, in the history of the individual gradually increase in complexity of organization as necessitated by the reaction to an increasingly complex environment.

These neuron systems are complex because they are integrated from earlier and more simple neuron systems. This complexity makes for variability and as potentially they become more variable, in proportion they become less stable. Further their functioning, as a complex system, necessarily implies a greater neuron activity or energy than do the lower systems.

If then, by reason of their potential variability, they be more unstable, and if, from any cause there be an insufficient neuron activity in the functioning of these systems, then this neuron activity, insufficient for higher functioning, tends to function the lower elements of these unstable systems with the production of motor equivalents of a correspondingly low order, which motor equivalents, in their mal-adaptation to the environment, in their involuntary character, present those characteristics observed in chorea.

Thus, whatever be the fundamental pathological cause of chorea, it is not simply a cortical irritation ; rather the pathology is one which distinctly concerns the motor neurons as systems. That the results of the pathological process are not displayed as irritative neuron discharge ; that rather analysis has shown them to be such as would result from the inhibition of the physiological functioning of higher systems, coincident with a hyperfunctioning of the motor neuron systems subordinate to the higher, from which the higher has been evolved and integrated.[1]

[1] The MS. of this article was received June 3, '05.—Ed.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF MOUNT HOLYOKE COLLEGE.

COMMUNICATED BY HELEN B. THOMPSON.

The Effect of the Brightness of Background on the Extent of the Color Fields and on the Color Tone in Peripheral Vision.[1]

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I. Preliminary Remarks.

Although considerable work has been done on the color-vision of the peripheral portions of the retina, its purpose has been, in almost every case, the determination of the relative size of the various color fields, and incidentally of the characteristic changes which take place in most colors as they pass from the center of the field of vision to its outer limits.

Any discussion of the work done with spectral colors in a dark room may be omitted here as irrelevant, since by this method there is no chance for change of background without introducing illumination of some sort.

The work which has been carried out by daylight falls naturally into two classes, one in which the results have gone to prove the coincidence of certain fields according to the Hering hypothesis, and another in which they have pointed to exactly the opposite conclusions. The two most recent and thorough investigations, those of Hess and Kirschmann, represent the two extremes — Hess[2] found the limits for blue very nearly coincident with those for yellow and the limits for green

[1] The greater part of the work reported in this paper was done in the psychological laboratory of Mount Holyoke College during the year 1903–4, but some additional tests have been made in the laboratory of Bryn Mawr College. I am indebted to Professor James H. Leuba, of Bryn Mawr College, for many valuable suggestions and criticisms.

[2] Hess, C., 'Ueber den Farbensinn bei indirectem Sehen, *Graefe's Archiv f. Ophthalmologie*, XXXV., 4, 1899.

very nearly the same as those for red, while Kirschmann[1] found no such correspondence.

The difference in results in the two cases might easily be due to the great difference in the conditions under which the experiments were performed. Hess worked with colors of equal saturation and brightness and insisted that the background must match the colors in brightness. While the eye followed a small moving fixation point the color was exposed at intervals.

Like Hess, Kirschmann used the campimeter method to avoid the unevenness of illumination, which is found with the perimeter. His campimeter consisted of a vertical strip which was set up so that the stationary fixation point at its center was on a level with the eye of the observer. The fixation point was a square whose side measured 5 mm. The head position was determined only by means of a screen fastened up in front of the vertical strip of the campimeter. This screen had two openings into which the eye and nose of the observer fitted. Thus, to some extent, eye and head movement were allowed. Kirschmann claims that this is not as serious a matter as the exhaustion caused by a more rigid holding of the fixation point, and the discomfort due to a close fitting head rest. He minimizes the possibilities of error by having a considerable distance between the eye of the observer and the fixation point. Hellpack discredits entirely Kirschmann's attempt to calculate exactly the amount of the error which could possibly be caused by eye movement and points out the fact that there was little more reason to believe that the eye would keep within wider bounds than within narrower ones.[2] In fact, it would be even easier unconsciously to move the eye beyond the limits than where the fixation point was so small that any movement could easily be detected.

The color, instead of the fixation point, was moved along the line of the campimeter. Kirschmann considers any error due to change in size of the retinal image because of the increasing distance of the object from the eye, less than that

[1] Kirschman, 'Die Farbenempfindung im indirecten Sehen,' *Philos. Stud.*, VIII., pp. 592–614.

[2] 'Die Farbenwahrnehmung im indirecten Sehen,' *Philos. Studien*, Vol. 15, 1899, p. 533.

which would be caused by the strain of eye movement if the eye followed a moving fixation point and says that the amount of the change in size could easily be calculated. (His procedure involves the continued exposure of the color during each test.) He criticizes the method used by Hess because the error due to irregularity and to after-imagery is of such a nature that it cannot be accurately calculated. He admits that in his own work there will be some error due to irregularity of movement and hence to faulty registration. This however he thinks can be remedied to a large extent by practice on the part of the experimenter. He passes over the effect of exhaustion due to continued exposure of the color with the mere mention that at each moment the color falls on a fresh portion of the retina. In reality only a very small part of the whole portion stimulated at any given moment is entirely unexhausted. This fact would not call for consideration if the work were all done in the inner color zone, but in the peripheral regions where there is little sensitiveness to color, slight exhaustion might easily make a decided difference in the extent of the color field or in the tone of the color seen.

An accurate comparison of the limits obtained by Hess and by Kirschmann with those determined in our own work is impossible, because of the disparity in the conditions under which the experiments were performed. Hess used physiologically pure colors of reduced saturation matching each other and the background in brightness, while we used colors of much greater saturations, not always physiologically pure, and very different in brightness. Kirschmann's work differed from ours in three particulars: (1) He used more than one shade of each color, taking his limit as the mean between the various points thus determined. (2) His black background was much darker than our darkest background, so that the only case in which our backgrounds were similar was that in which the white background was used. With the white background only one subject (himself) was experimented upon, so that individual peculiarities may explain the difference in results even with similar backgrounds. (3) As Kirschmann moved the color along the plane surface the difference, between the size of the

retinal image in his experiment and ours, would vary at different points. For instance, in his work with the white background the retinal image would be at least twice as large as ours for the point on the perimeter where green was last seen, and somewhat larger than ours where blue was last seen. Though the difference in size could be calculated for each limit there seems as yet to be no way of accurately estimating the difference in effect due to increase or decrease in the size of the retinal image. These differences in method are mentioned only in order that we may not seem too dogmatically to ascribe the considerable divergences which exist between the limits determined by Kirschmann and those determined in our work, to difference in the method of exposing the color.

The fact that Kirschmann's limits are, in every case, considerably narrower than ours is at least not contradictory to the statement that exhaustion due to continued exposure of the color decreases the size of the retinal color fields, and it is entirely possible that his fields would have been wider had he eliminated this possibility of error.

In class work done here according to the method described in Titchener's *Manual* (pp. 9–11) the students constantly complained that when they started with a fixation point well beyond the limits found for a color by moving the eye out in a centrifugal direction, the color was clearly seen when the fixation point was first taken, and lost only by holding the point an appreciable length of time. Then if the eye was moved in, the color next became perceptible at a point farther in than the one at which it had disappeared in a previous test. Since the difficulty could not be removed by the most careful holding of the fixation point and by constant practice, a series of tests was made to find how far out on the retina it was possible to see colors when there was no retinal exhaustion. To do this a method was devised which did away largely not only with retinal fatigue, but also with eye movement during a test.

The details of the method will be described when we discuss our own experiment. For the present it will be sufficient to say that it consisted essentially in first taking the fixation point and then exposing the color, giving a sufficient rest interval between

tests. The fields determined in this way were considerably wider than any we were able to obtain by a method which involved continued color exposure or eye movement. An additional advantage of the method is that it makes possible a study of the changes which may take place in a color when exposed for any length of time at a given point on the retina.

While both Hess and Kirschmann worked with backgrounds of different brightnesses, neither of them takes up the question of the effect of the background on the color tone.

In the two places where Hess remarks particularly upon the effect of the brightness of background on the color, he makes it very clear that the only changes he considers as taking place are those in brightness and in saturation of the color. After he has enumerated the various alterations which may take place in the color tone of a given color in different portions of the color field, he says (I., p. 7): "In the previous summary notice is purposely taken only of the color tone. The changes in saturation and brightness demand a special discussion. In this respect one and the same pigment color can go through very different changes. For example, in indirect vision, a saturated orange on a black background is seen as bright yellow, while on a white background, it is seen as dirty dark yellow. A given green, moreover, with all other conditions constant, can be seen as dark gray on a white background, as light gray on a black background, and can be entirely invisible on a gray background of a certain brightness."

Later on in stating his reasons for insisting that the white valency of the color and of the background must be the same if the measurements of the color fields are to be accurate, he says that the color is seen farthest out on the periphery when the color and background match in brightness. (I., p. 51.) "In indirect vision, the relation between the white valencies of the color and the background has a decided influence on the perceptibility and upon the disappearance of the color. The color is perceived farthest out when the background is of the same white valency as the color. As the difference between the two white valencies increases, the peripheral color field decreases." He cites as an illustration an experiment in which the limit for

physiological red along the temporal meridian was 27° with a matched background, 19° with a black background, and only 13° with a white background.

Kirschmann, who, as has been stated, worked only with black and white backgrounds, found decided difference in the relative size of the color fields in the two cases. The only qualitative difference mentioned by Kirschmann as directly due to the background is found in the case of yellow and is especially interesting to us. His statement is as follows (II., p. 607): "Our yellow disk, and sometimes even the yellow-green disk, arouses the orange perception on the nasal meridian almost sooner than the orange color itself, since the latter probably loses something through brightness contrast with the white background. Apparently for the same reason the specimens of the lighter less saturated series were frequently recognized sooner than the real representatives of the colors concerned." This peculiar behavior of yellow with the light background is in perfect agreement with our results, though our explanation will be seen to differ decidedly from that given by Kirschmann. We found yellow frequently seen as orange within the limits for orange and often beyond the place where the orange disk itself could be correctly perceived.

As Kirschmann was not testing the difference in the effect of the two opposite backgrounds on the color fields, he varied the conditions with respect to size of retinal image and number of colors used in the two cases, so that we will not attempt to compare the two sets of limits further than to say that with the light background his yellow field is strikingly small on the nasal meridian.

II. Experimental Work.

The purpose of the experiment was to determine (1) the dependence of color tone on the brightness of the background and (2) the variations which take place in the size of the individual fields with backgrounds of different brightnesses. Since no attempt was made to establish the relative size of the fields of different colors, but only to determine the difference in the behavior of one and the same color under different conditions,

it was not necessary to fix an equal standard of saturation and brightness for the various colors used. It was essential, however, that the only changes occurring should be due to the difference in background and not to any other factors. The general conditions of the experiment had, therefore, to be made as constant as possible.

The main sources of error to be avoided were, changes in illumination during a given experiment or from day to day, after-imagery and exhaustion due to the presence of any color beside that used in the experiment, exhaustion due to any other factor, as continued exposure of the color or an uncomfortable position on the part of the observer, and finally the difference in eye position for a given fixation point.

That the illumination might be kept, as far as possible, constant and also that there should be no foreign color effect, the work was all carried out in a room whose walls and woodwork were of a uniform white. The table on which the campimeter stood was covered with a medium gray cloth, and heavy white curtains were arranged at the windows so that they could be lowered on bright days and drawn back on dark days. Care was taken not to work when the illumination was really poor.

The remaining sources of error all have to do with the method of exposing the color and of determining the eye position during the exposure. Authorities differ widely as to the best method — since it seems as if one error is no sooner escaped than another one is introduced.

The methods employed by Hess and Kirschmann have already been briefly described, the difference between these two experimenters being that while Hess used a stationary color exposed only at intervals and a movable fixation point, Kirschmann moved the color along the campimeter and had a stationary fixation point.

The question of error due to difference in head position has never been considered a serious matter, and yet it can readily be seen that with any sort of a head rest, which did not clamp the head into an impossibly uncomfortable position, the head might be so placed at different times that the eye position with reference to the opening of the campimeter or to the color

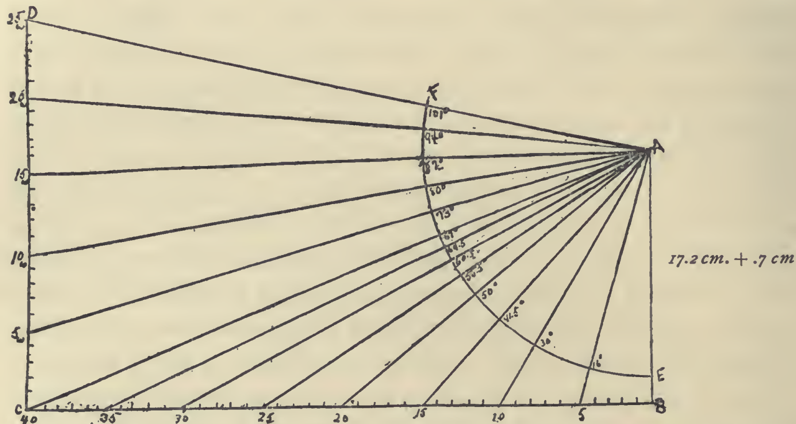
might vary considerably. That is, the angle of the visual axis with the plane of the campimeter might differ at different times and yet the observer have the eye fixed on the same point, so that for a given fixation point the image of the color might fall on different portions of the retina.

The method of the present experiment varied somewhat from those of either Hess or Kirschmann. The apparatus used was a horizontal campimeter. The head was supported on a rest made of lead which was soft enough so that it could be bent to fit the forehead and cheek of the observer, and yet hard enough to keep its shape when once bent. (A different head rest, with an opening on the side toward which the eye turned, was used for each principal meridian.) The rest was firmly supported on iron standards and tested frequently to be sure that it remained at the same distance from the opening. The right eye was used in all the experiments and the left eye was covered with a small black cap which allowed the eye to remain open. Since, in the case of the nasal meridian, no point on a level with the campimeter was found at which all the colors failed to be seen, a vertical strip was attached to the edge of the campimeter farthest from its circular opening at right angles to its horizontal plane. By taking a fixation point on this upright strip the eye could be turned so that the color would fall on the extreme edge of the field of vision. The campimeter readings could readily be transformed into degrees on the retina by some such simple device as that shown on opposite page.

In order to insure constancy in eye position with regard to the color exposed a small square of mirror as near colorless as we could obtain, was placed on the table directly beneath the opening of the campimeter.[1] For each test the observer began with his eye looking directly into the mirror, with the long axis of the opening of the eye in line with the fixation points lengthwise of the campimeter, and then moved it out to the given fixation point. As, at the start, the eye was so placed above the opening of the campimeter that the image of the pupil in the

[1] This mirror was held in position by thumb tacks and leveled before each experiment by means of a circular level. It was also tested at the end of each experiment to make sure that it had not changed level.

mirror appeared equidistant from the sides of the opening, there was little chance for differences in the position of the eye with reference to the color, when the eye had moved to its fixation point. That is the angle between the visual axis and the plane of the campimeter was a right angle when the eye was fixed on its own image in the mirror. For any other given fixation point the acute angle formed between the line of the visual axis and the plane of the campimeter was always the same and the color fell at the same distance from the center of the retina.



A represents eye position, *B* the opening in the campimeter (opening 1.2 cm. in diameter); distance from eye to campimeter 17.2 c.c., and from the cornea to the nodal point of the eye .7 cm. *BC* represents the line on the base of the campimeter and *CD* the line, on the vertical piece, at right angles to *BC*. The divisions on the lines *BC* and *CD* represent divisions of 1 cm. each, and those on the arch *EF* degrees on the retina. Suffix *u* stands for upright.

After the observer had taken the fixation point, the color concealed beneath a gray paper which matched the background in brightness, was slipped over the mirror and the cover quickly removed. As the color was exposed a stop-watch was started, and stopped at a signal from the observer, when the color was gone. The observer, who had no previous knowledge as to what the color was to be, reported what he had seen. An interval of two minutes was allowed before each test, to avoid exhaustion effects.

Now the strongest proofs that the colors seen with the distant fixation points were not due to eye movement, are the results of these experiments. In the first place, at the very outer limits where color was first distinguished, it was generally seen as a dull or, less frequently, as a bright flash of color, often different in color tone from the original stimulus. With the darker backgrounds the various greens and reds, including orange, were first seen as yellow, while, with the light background, orange was seen as a bright flash of pure red. Then, as the color was brought farther in toward the center of vision, the correct color tone began to be seen and a gradually increasing time during which the color was seen, was found. This was too constant to be disregarded. The following readings for orange show the gradual increase in time, and are typical illustrations of the time measurements. The orange exposures were not given in immediate succession as the table seems to indicate, but were separated by considerable intervals in which other colors were tested.

BACKGROUND NO. 7. COLOR ORANGE.

Fixation Point.	Color Seen.	Time.
20_u	Chiefly white—perhaps yellow.	
19_u	Very brief flash of yellow.	——
18_u	Brief flash of yellow.	——
15_u	Yellow.	1.2 sec.
14_u	Yellow, slightly orange tinge.	3.2 "
11_u	Yellowish orange.	3.4 "
8_u	" "	4 "
2_u	Orange changed to yellow.	4 "
4_u	Reddish orange.	6 "

(The suffix *u* means that the fixation point was on the upright of the campimeter. The readings are in centimeters.)

The following averages are taken from the preliminary work done with red. The conditions of the experiment were the same as those in the final work except that red was used almost exclusively. This gives a much greater number of readings for each average than could be obtained from our later work.

The index shows in each case the number of readings included in an average. The numbers of the table designate the subjects experimented upon, and in each case refer to the

same subjects as those marked I. and II. in the tables at the end of the paper. An entry 0 means either that the color was not seen or else appeared in a flash too brief to be measured by the stop-watch. The time readings are all in seconds.

SUBJECT NO. I.

Color = Red. Nasal Meridian. Background = Hering Gray Paper No. 17.

	20_u	19_u	18_u	17_u	16_u	15_u	14_u	13_u	12_u	11_u	10_u	9_u	8_u
Not seen.	0^3	0^2	0^2	0?	0?	0^2							
Seen as yellow.	$0'$	0?	0?	$0'$	0^3	0^6	$0'$	1.5^2					
" " orange.				$0'$		0^2	$0'$	1.5^2	1.8^2		3.3^{10}	4.1^5	$2.5'$
" " reddish orange.								1.4^2			$2.8'$		$1.9'$
" " red.										$0'$			

	7_u	6_u	5_u	4_u	3_u	2_u	1_u	40	35	30	20	18
Not seen.												
Seen as yellow.			3.6^2									
" " orange.	5.1^3		4.7^4	$4.2'$				$7'$	5.3^2			
" " reddish orange.		2.6^3	4.3^5	$3.4'$	5.4^2	4.3^3	5.5^2	5.9^4	5.7^5	5.7^5	7.4^2	
" " red.											14?	$10'$

SUBJECT NO. II.

Color = Red. Nasal Meridian. Background = Hering Gray Paper No. 17.

	24_u	23_u	22_u	21_u	20_u	18_u	17_u	16_u	15_u	14_u
Not seen.	0^4	0?	0^2	$0'$	0^2	0^3			0^2	$0'$
Seen as yellow.	0y′	0^1	0^4	0^2	3.5^4	3.5^7	$5'$	4.9^2		
" " orange.	0?		$0'$		0^2	$0'$	$0'$			6.5^3
" " reddish orange.								$3'$		
" " red.				$0'$	0^2			$0'$	0^3	$0'$

	13_u	12_u	11_u	10_u	9_u	8_u	7_u	6_u	5_u	4_u
Not seen.										
Seen as yellow.	11.1^2									
" " orange.		6.9^3	$10.2'$		$10.2'$	$6.8'$	$6'$			
" " reddish orange.		$7.2'$	$7.2'$	5.5^2		6.2	$5.6'$	8.2^2	6.4^2	10.6^2
" " red.	$0'$	$0'$	0^2	2.3^3	0^2	$4.6'$		7.9^2		6.6^2

	3_u	2_u	1_u	40	35	30	20	10
Not seen.								
Seen as yellow.								
" " orange.			$8.2'$					
" " reddish orange.	7.1^3			8.6				
" " red.		8.9^4	$9.6'$		$7.2'$	$11.2'$	$20'$	$28+'$

The results show the gradual increase of the time during which the color is seen as the color moves in from the extreme peripheral regions toward the center of vision. They also show

that, in certain peripheral portions of the retina there are three possibilities for red at the same point — either the true color tone is seen for a briefer time, or else yellow or orange appears and remains for a longer time. It has been found in all the work that there is a strong tendency for a color to last longer at the inner limits of the zone where it is seen as yellow than at the outer limits of the zone where it is seen in its true color tone.

To determine the effect of the brightness of background on color tone, blue, yellow, red, green, and orange have been tested with four different backgrounds. The colors used were the Hering disks (tissue-paper series) representing a fully saturated spectrum. The backgrounds were of Hering gray paper which matched the yellow, the blue, the green, and the red disks in brightness. Arranged according to brightness their order would be — the background for blue, Paper No. 29[1] (a very dark gray); for red, Paper No. 17; for green, Paper No. 7; and for yellow, Paper No. 3 (a very light gray). The method for matching the brightness of the background to the colors was that of exposing the color in the usual way at a point in the periphery beyond the limits of color vision, and determining the brightness at which each color remained entirely invisible when so exposed.

The effect on color tone of a change in the brightness of the background is twofold: (1) It modifies the tone of the color as initially perceived, (2) it affects the change taking place in the color tone as the color fades out.

In the tables at the end of the paper the Arabic numerals represent the color seen when no change in color tone took place as the color disappeared, the Roman numerals represent the color seen when there was a change in color tone during the exposure. In every case except that of green this change is toward yellow. Green, whenever it changed tone, began with a yellow and grew either greenish-yellow or pure green. The Arabic numerals in heavy-faced type mean that no color was seen at the point designated.

[1] Paper No. 34 was found to match better in later tests.

The four persons who have been experimented upon have all had previous training in laboratory work. Two of them, I. and II., knew what results were being obtained, while III. and IV. knew nothing whatever about the experiment or the colors used.[1] The general character of the results differs very little in the two cases.

Only those colors have been worked with which change their tone toward yellow rather than blue in peripheral vision. It will readily be seen that in general the change is toward yellow with the dark background, and toward red or green with the light backgrounds. The following tables summarizing all the results of the final work show the characteristic behavior of the colors:

Nasal Meridian.

Color.	Background.	Seen as Red.	As Orange.	As Yellow.	Not Seen.	Total No. Tests.
Red.	For blue.	19 XVII.	16 IX.	10	3	74
"	" red.	37 XIV.	13 VII.	13	14	98
"	" green.	39 I.	3 II.	6	19	70
"	" yellow.	65	4		15	84

Color.	Background.	Seen as Orange.	As Red.	As Yellowish-Orange.	As Yellow.	Not Seen.	Total.
Orange.	For blue.	XXX. 2		XI. 10	31	3	84
"	" red.	VI. 13	II. 15	VIII. 12	28	8	75
"	" green.	XII. 19	II. 100	23	8	8	100
"	" yellow.	24		11	3	16	156

Color.	Background.	Seen as Yellow.	As Orange.	Not Seen.	Total.
Yellow.	For yellow.	88	32	16	132

Color.	Background.	Seen as Green.	As Greenish-Yellow.	As Yellow.	Not Seen.	Total.
Green.	For blue.	7	8	15	3	33
"	" red.	13	25	23	14	75
"	" green.	29 III.	20 IX.	30	16	107
"	" yellow.	14 I.		2	29	46

Temporal Meridian.

Color.	Background.	Seen as Red.	As Orange.	As Yellow.	Not Seen.	Total.
Red.	For blue.	13	8 I.		3	25
"	" green.	10 I.	3		5	19
"	" yellow.	21			2	23

[1] IV. was unable to continue the experiment after the readings on the nasal meridian had been taken.

Temporal Meridian.

Color.	Background.	Seen as Orange.	As Red.	As Yellowish-Orange.	As Yellow.	Not Seen.	Total.
Orange.	For blue.	7 XI.		8 VIII.	15	10	57
"	" green.	18 I.	2 1?	7 I.	8	6	44
"	" yellow.	7 7R.O. (2?)	14 II. (1?)	2 (2?)	4	6	47

Color.	Background.	Seen as Green.	As Greenish-Yellow.	As Yellow.	Not Seen.	Total.
Green.	For blue.	15 V.	5 II. (1?)	14	6	48
"	" green.	10 (1?)	17	1	8	37
"	" yellow.	23 (2?)	3	1	6	35

Upper Meridian.

Color.	Background.	Seen as Red.	As Orange.	As Yellow.	Not Seen.	Total.
Red.	For blue.	20 I.	29	3	3	56
"	" green.	30	9		22	61
"	" yellow.	44			15	59

Color.	Background.	Seen as Orange.	As Red.	As Yellowish-Orange.	As Yellow.	Not Seen.	Total.
Orange.	For blue.	10 V.		6 VII.	16	7	51
"	" green.	42	19	11 I.	10	15	98
"	" yellow.	25	28			8	61

Color.	Background.	Seen as Green.	As Greenish-Yellow.	As Yellow.	Not Seen.	Total.
Green.	For blue.	15 II.	11 II.	14	12	56
"	" green.	10 I.	19 (2?)	13	21	66
"	" yellow.	7	7	2	4	20

Lower Meridian.

Color.	Background.	Seen as Red.	As Orange.	As Yellow.	Not Seen.	Total.
Red.	For blue.	8 I.	15 (1?)	2 1?	2	30
"	" green.	14			12	26
"	" yellow.	6	1 (1?)		4	12

Color.	Background.	Seen as Orange.	As Red.	As Yellowish Orange.	As Yellow.	Not Seen.	Total.
Orange.	For blue.	5 I.		8 III.	5	8	30
"	" green.	17	4	1	2	10	34
"	" yellow.	9	11 II.		1	3	26

Color.	Background.	Seen as Green.	As Greenish-Yellow.	As Yellow.	Not Seen.	Total.
Green.	For blue.	2	5	9	6	22
"	" green.	2	7	7	5	21
"	" yellow.	7 (1?)		1	4	13

Red appears as orange or yellow only with the three darker backgrounds and much less often as one of these colors with the background No. 7 than with the backgrounds No. 17 and No. 29, which are darker. It is also true, with a few exceptions (which occur with the background No. 7), that red changes its color tone toward yellow when seen first as red, only with the two darkest backgrounds. On the background No. 3 it is either not seen, or is seen as red at the same points where it is seen as yellow or orange with the two darker backgrounds.

Orange shows even more clearly than red the effect of change of background. With the two darkest backgrounds it is almost invariably seen as yellow or yellowish orange at the farther fixation points. Even when it is seen as orange at the more central points it fades out as a clear yellow. With the medium background (No. 7) there seems to be a sort of transition stage. That is orange is sometimes seen as orange, sometimes as yellowish-orange and yellow, and sometimes as a very reddish-orange or a pure red when this background is used. With the lightest background orange is oftenest seen as pure red.[1] The color as described by all the observers is 'a good pure red just like the red disk.' It is practically never seen as yellow with this background.

Green is less certain in its behavior and, unfortunately, the readings for green are less complete than those for orange and red. There seems to be a much stronger tendency for green to be seen as yellowish-green and yellow with the three darker backgrounds than with the lighter background. This is certainly the conclusion to which the readings on the nasal and temporal meridian point. Even with the other two meridians, where the proportion of readings for green is small, the results favor the statement just made. The darker backgrounds certainly intensify the yellow in the pigment color more than the lighter backgrounds.

[1] This result is at variance with that of Hess, who affirms that orange looks a 'light yellow' with a dark background and a 'dark yellow' with a light background. He gives only a few results obtained with the black and white background, and most of his work seems to have been done with the medium gray background. This may explain why his results differ from ours.

Yellow is seen as orange only with the background No. 3, and is frequently judged to be orange along all the meridians with this background. It ought to be stated here that the yellow disk used belonged to the older set of Hering disks, and is a somewhat golden yellow though by no means enough so to be called orange.

It would seem, in consideration of the results given above, as if the color most like the background in brightness was the one which tended to be obliterated. With the background for red, and also that for blue, the red fails to have its full effect in determining the color seen, while the yellow, which is farthest from the dark backgrounds in brightness, has its effect in some way heightened by the brightness contrast. Thus beyond the limits for red, orange and green we see a clear, distinct yellow when either the red or green disks are used. With the background for yellow we find red seen only as red, orange with its red factor so intensified and its yellow factor so lessened, that it is often seen as red, and even yellow tends to appear as orange.

The increased activity of yellow with the darker backgrounds and the lessened activity of green and red is seen in the fact that even within the limits for these colors, the yellow factor is predominant either before or after the true color is seen, and the interval during which the true color tone is distinguished is short as compared with that when the lighter backgrounds are used.

If it is true that the color least like the background in brightness is intensified, we would expect to find a decided difference in the width of color fields with a change in the brightness of the background. So far the only fields we have carefully determined are those for blue and yellow on nasal meridian. Tables I. and II. are the results for the same two subjects as those designated in the other charts as I. and II. Subject III. is a graduate student at Bryn Mawr College who knew nothing of the experiment. In the last two cases the darkest background used was No. 34 instead of No. 27, and the medium background was No. 17 instead of No. 7.

The field for yellow is decidedly widened in every case as the background grows darker. This fact seems to agree very well with the qualitative effects of the background on the color.

Limits for Yellow. *Nasal Meridian.* *Limits for Blue.*

Limits for Yellow.

I. Background No. 29. Degrees on Retina.	Seen.	Not Seen.	No. 7. Seen.	No. 7. Not Seen.	No. 3. Seen.	No. 3. Not Seen.
98.5	3	11				
97	3	1[1]				
95.5	3	1				1
94	1			3		4
92.5	2		2?	3 green	1	2
91.5	1		4	1[1]	1	4
90			3		2	1
88.5			1	1	4.1?	[1]
87					1 3 orange	1?

II. Background No. 34.	Seen.	Not Seen.	No. 17. Seen.	No. 17. Not Seen.	No. 3. Seen.	No. 3. Not Seen.
98.5	1	2		3 1?		4
97	2	2 ⋮ 2?	3	5 ⋮ 1?		7 1?
95.5	7	2[1]	4	3 ⋮ 1?	3	7
94	6 ⋮ 1?	1	6	[1]	1 ⋮ 3?	4 ⋮ 1?
92.5	2		2		4	2[1]
91.5	2				2	2

III. Background No. 34. Degrees on Retina.	Seen.	Not Seen.	No. 3. Seen.	No. 3. Not Seen.
102.5		4		2
101	4 2?	4	1	6 1?
99.5	7 1?	1?[1]	4	9
98.5	6 2?	1	4 4?	3 1?
97	6 1?		7 2 orange	1?
95.5	4		6	4
94	4		2 1?	2

Limits for Blue.

I. Background No. 29. Degrees on Retina.	Seen.	Not Seen.	No. 7. Seen.	No. 7. Not Seen.	No. 3. Seen.	No. 3. Not Seen.
91.5	1	3		4		1
90	1 1?	3		3	1	1
88.5	3 1?	1[1]	1 1?	2	1	3
87	2		4	1[1]	2	
85.5	1		2 1?	1	4	4
84			2		2 1?	5
82.5			3		5 1?	5
81					6	

II. Background No. 34.	Seen.	Not Seen.	No. 17. Seen.	No. 17. Not Seen.	No. 3. Seen.	No. 3. Not Seen.
101						2
99.5					1	1?
98.5		3		3 1?	4?	4[1]
97	1	4 1?	3	4 3?	6 2?	1 1?
95.5	6 1?	4[1]	5	1[1]	4 2?	2
94	6 1?	1? 1	5		9	
92.5	3		2		6	
91.5	2				5	

III. Background No. 34. Degrees on Retina.	Seen.	Not Seen.	No. 3 Seen.	No. 3 Not Seen.
101		3		2
99.5	3	5 1?		6
98.5	2 1?	4 1?	2	6
97	5 2?	1[1]	7 1?	2[1]
95.5	4	1	5 1?	1?
94	2 1?	1	5	1

[1] Indicates the point taken as the limit in each case.

For blue the results are not so conclusive. In table I. blue is seen farther out with the dark background than with the light background. The explanation here might be that the observer evidently has not as keen a perception of blue as of yellow and so is more erratic in the case of blue. It is impossible to tell as the table stands whether the limit with the lightest background would be at 82.5°, at 85.5°, or at 87°. If 87° is the correct limit then the results are not especially disturbing.

In the second table the limits for blue are the same with the two darker backgrounds, but wider with the lighter background. In table III. the limits are exactly the same with the two backgrounds used.

In the work represented by the tables at the end of the paper, the most complete set of results was obtained with orange on the nasal meridian. It will be seen from these results that there is a strong tendency on the part of orange to appear in its true color tone much farther out with the lighter backgrounds than with the darker backgrounds. In those cases in which it is not seen farther out as orange, it is seen farther out as red, which means simply that the red factor is intensified. That is, orange will be seen as *yellow* with the *darker backgrounds* at the same fixation points where it is seen as *red* or *orange* with the lighter backgrounds.

The following table gives the farthest points at which orange was seen as red, orange or yellow with the various backgrounds.

Limits for Orange—Nasal Meridian.

I.

Background No. 29.	Seen as *Orange* to	73°	As *Yellow* to	87°
" " 17.	" "	69.5°	"	92.5°
" " 7.	" "	73°	"	92.5°
" " 3.	" "	84°	As *Red* to	87°
			(once only as yellow)	

II.

Background No. 29.	Seen as *Orange* to	64.5°	As *Yellow* to	94°
" " 17.	" "	60.5°	"	94°
" " 7.	" "	82.5°	As *Yellow* to	91.5°
			As *Red* to	72°
" " 3.	" "	85.5°	"	91.5°

III.

Background No. 29.	Seen as *Orange* to 72°	As *Yellow* to 91.5°
" " 17.	" " 78.5°	" 91.5°
" " 7.	" " 80°	As *Yellow* to 91.5° As *Red* to 64.5°
" " 3.	" " 82.5°	" 88.5°

IV.

Background No. 29.	Seen as *Orange* to 67° (once at 87°)	As *Yellow* to 90°
" " 17.	Seen as *Orange* to 64.5°	" 94°
" " 7.	" " 64.5° (once at 87°)	As *Yellow-orange* to 88.5° (once only as yellow) As *Red* to 77° (once only as yellow)
Background No. 3.	Seen as *Orange* to 74.5°	As *Red* to 84° (once only as yellow)

The effect of the brightness of backgrounds on the limits for red and green along the nasal meridians and for all the colors on all the other three meridians cannot as yet be reported upon. Our results are too incomplete to justify any further conclusions. Orange was used more often than the other two colors because of its peculiar interest.

As far as the work has been carried out we seem justified in drawing two conclusions:

1. The brightness of a colorless background has a decided effect on the color tone of any color which is not spectrally pure — that is on any color which is, in the slightest degree a color mixture as is the case with all pigment colors. It appears further that this effect may be due to the intensifying of the component color least like the background and the obliterating of the component color most like the background in brightness.

2. The brightness of the background affects also the width of the color field. In the case of orange and yellow the field is widest when the contrast between the brightness of the color and of the background is greatest. The explanation of this fact with regard to orange must again be based upon the fact that orange is a color mixture, since what seems to occasion the change in size of the orange field is the relative difference in the proportions of the red and yellow factors with the different

backgrounds. The question would ultimately reduce itself to the effect of the background on red and yellow respectively. If orange were taken as one color it would seem that it ought to be distinguished farther out with a very dark background than with a very light background, since in the case of the former background the brightness contrast between color and background is really greater than in the latter case. But if the brightness of the background effects the two components of orange differently, emphasizing in every case the color least like the background in brightness, then the results obtained are in exact agreement with what we should expect. It is possible that further work will show a change in the limits for blue in accord with the principle which seems to apply to yellow and orange, though it scarcely seems probable that the effects will be very pronounced.

We are at present at work on the colors that change their color tone toward blue (*i. e.*, purple, violet, carmine, blue-green and green-blue), and also on the retinal limits of all the colors represented in the Hering disks. The apparatus has been so modified that the mirror is no longer necessary for determining the initial eye position, but the results so far seem to agree with those already reported.

A further point worth mentioning is the fact that, in the case of several colors, exposure, beyond the limits where any color is seen, is followed by a very clear after-image. This was repeatedly found to be true with red, orange, green, and blue and often with yellow. This after-image for the first three and for yellow was blue, and for blue a very clear yellow. This may explain the 'gegen farbige' zone[1] found by Hellpack in his dark-room work, as under those conditions there would have been no way of telling whether the color came exactly at the time of exposure or immediately afterwards.

Explanation of Tables.

The numbers of the tables indicate the subjects, the letters the meridians: (A) nasal, (B) temporal, (C) upper and (D) lower.

Arabic numerals designate color seen without change of tone during exposure.

[1] *Phil. Stud.*, 1899, Vol. 15, p. 536.

Roman numerals designate color seen with change of tone during exposure.

Arabic numerals in heavy-faced type indicate that no color was seen.

To trace the limits for a color it is necessary, first, to find the points where no color was seen, then where it is seen in other color tones than its own, and lastly where the true color was seen. It may sometimes appear as if no points beyond the limits for the color had been determined when it will be found that there is a wide zone, beyond the real limits for the color where it is seen as an entirely different color. For example, orange with the dark backgrounds would have an outer zone where it would not be seen as a color, a second zone where it would be seen as yellow, a third where it would appear yellowish orange and lastly one where its true color tone would be distinguished.

TABLE III D. Lower Meridian.

Background.	Color.	Color Seen.	Degrees on the Retina.											
			13-22	25	27.5	30	33	35	37	39	41.5	43	45	47
For blue.	Red.	Red.						I			I	I		**1**
" green.	"	"									I	2	**1** 3	**1**
" yellow.	"	"								I			**1**	
For blue.	Red.	Orange.			I		I		I		I	I?	I	2
" green.	"	"												
" yellow.	"	"										I?		
For blue.	Orange.	Orange.			I									**2**
" green.	"	"			I					I	I	2	**1**	**2**
" yellow.	"	"	3			I	I			I				**1**
For blue.	Orange.	Yellowish-orange.								I	I.	I	I I.	
" green.	"	"												
" yellow.	"	"												
For blue.	Orange.	Yellow.											I	
" green.	"	"												
" yellow.	"	"					I	I	I					
For yellow.	Orange.	Red.	2	I									I	
For blue.	Green.	Green.						**1**	**1**			**1**		
" green.	"	"									**2**			
" yellow.	"	"	I	I		I		I	**1**	**1**				
For blue.	Green.	Greenish-yellow.								I				
" green.	"	"						I						
" yellow.	"	"												
For blue.	Green.	Yellow.			2						I			
" green.	"	"									I			
" yellow.	"	"					I							

TABLE I A. NASAL MERIDIAN.

Background.	Color.	Color Seen.	Degrees on the Retina.															
			10-39	41.5	43	47	50	55	55.5	57	60.5	62	62.5	63.5	64.5	67	68	69.5
For blue.	Red.	Red.	3	II.			2	2	1 I.		1 I.				I.	1		1
" red.	"	"														I.		
" green.	"	"													2	2		
" yellow.	"	"	1	1		1	1				1				2	4		3
For blue.	Red.	Orange.					1				1 I.					1		1
" red.	"	"																
" green.	"	"																
" yellow.	"	"														1		1
For blue.	Red.	Yellow.								1								
" red.	"	"																
" green.	"	"																
" yellow.	"	"																
For blue.	Orange.	Orange.	II.	III.			V.		II.	V.					I.	II.		
" red.	"	"														I.		2
" green.	"	"											1	I.	3	2		I.
" yellow.	"	"		1			2				2				2	1		1
For blue.	Orange.	Yellowish-orange.				II.	1								2	1		
" red.	"	"														I.	I.	
" green.	"	"											1	I.				
" yellow.	"	"														2		
For blue.	Orange.	Yellow.																2
" red.	"	"																
" green.	"	"																
" yellow.	"	"																
For green.	Orange.	Red.																
" yellow.	"	"	3	2	1				2						2	1		3
For blue.	Green.	Green.							1		1							1
" red.	"	"														3		1
" green.	"	"									1	1				I.		I.
" yellow.	"	"									2				3 2	1 2		1
For blue.	Green.	Greenish-yellow.									1				1			
" red.	"	"											1		1			1
" green.	"	"											1		I.	II.		I.
" yellow.	"	"																
For blue.	Green.	Yellow.													2		2	
" red.	"	"																
" green.	"	"																
" yellow.	"	"																

TABLE I A. NASAL MERIDIAN.—*Continued.*

Background.	Color.	Color Seen.	Degrees on the Retina.																
			70.5	72	73	74.5	76	77	78.5	80	81	82.5	84	85.5	87	88.5	90	91.5	92.5
For blue.	Red.	Red.			1	1													
" red.	"	"			1			1.		2					1 1				1 1
" green.	"	"			1	2	3			2	2	1 1	1		1	1		1	
" yellow.	"	"			1	1	2			4 2		1	1	1 1	2 2				1
For blue.	Red.	Orange.			1					2			1						
" red.	"	"					1						1		1				
" green.	"	"					1												
" yellow.	"	"																	
For blue.	Red.	Yellow.				1						1		1					
" red.	"	"											1		1	1			
" green.	"	"																	
" yellow.	"	"																	
For blue.	Orange.	Orange.			1								1						1
" red.	"	"																	
" green.	"	"		1	1													1	1
" yellow.	"	"				1	1					1	1 1	1	1		1		
For blue.	Orange.	Yellowish-orange.																	
" red.	"	"										1.							
" green.	"	"			1					1	1								
" yellow.	"	"					1			1									
For blue.	Orange.	Yellow.			1					2		3		1	1				
" red.	"	"	1	1		1				1			1	2	2		2	1	1
" green.	"	"								1	1	1		2				1	3
" yellow.	"	"													1				
For green.	Orange.	Red.			1														
" yellow.	"	"		1	3					3		1	1		3				
For blue.	Green.	Green.																	
" red.	"	"			1		1			2		2				2	2		2
" green.	"	"											1	1	1			1	1
" yellow.	"	"			1		2			3					1	1		1	
For blue.	Green.	Greenish-yellow.																	
" red.	"	"		1		2				1									
" green.	"	"		1.	1.														
" yellow.	"	"																	
For blue.	Green.	Yellow.			2					1		1							
" red.	"	"			1	1		1		1		1	1		2		1		1
" green.	"	"			1		2	2			1		1	1	1		2		1
" yellow.	"	"																	

TABLE II A. NASAL MERIDIAN.

Background.	Color.	Color Seen.	Degrees on the Retina.											
			10–39	41.5	43	47	50	51	55.5	57	60.5	62	62.5	63.5
For blue.	Red.	Red.		I.			I.		II.					
" red.	"	"												
" green.	"	"		3			1		1		1			
" yellow.	"	"							1					
For blue.	Red.	Orange.									II.			
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For blue.	Red.	Yellow.												
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For blue.	Orange.	Orange.	I.	I.			I.							
" red.	"	"	2								I.			
" green.	"	"		2					2	2	I. 2			
" yellow.	"	"	2											
For blue.	Orange.	Yellowish-orange.							I.		II.			
" red.	"	"												
" green.	"	"		2										
" yellow.	"	"												
For blue.	Orange.	Yellow.												
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For green.	Orange.	Red.	1	1					I.					
" yellow.	"	"	2	2			I. 1		2		1	1		1
For blue.	Green.	Green.												
" red.	"	"												
" green.	"	"		1			1		2		2 I.			
" yellow.	"	"												
For blue.	Green.	Yellowish-green.												
" red.	"	"									1			
" green.	"	"												
" yellow.	"	"												
For blue.	Green.	Yellow.												
" red.	"	"												1
" green.	"	"												
" yellow.	"	"												

TABLE II A. NASAL MERIDIAN.—*Continued.*

Background.	Color.	Color Seen.	Degrees on the Retina.											
			64.5	67	68	69	70.5	72	73	74.5	76	77	78.5	80
For blue.	Red.	Red.	1.											
" red.	"	"		1				1	1					2
" green.	"	"	1	1				1				1		1
" yellow.	"	"												
For blue.	Red.	Orange.		2										1.
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For blue.	Red.	Yellow.	1											
" red.	"	"									1			
" green.	"	"												
" yellow.	"	"												
For blue.	Orange.	Orange.	1.											
" red.	"	"												
" green.	"	"	1	2					1					1.
" yellow.	"	"												
For blue.	Orange.	Yellowish-orange.	1.	1					1					1
" red.	"	"	1					1	1					
" green.	"	"					1			3				1.
" yellow.	"	"												
For blue.	Orange.	Yellow.		1										
" red.	"	"								1		1		1
" green.	"	"										1		
" yellow.	"	"												
For green.	Orange.	Red.	2					1						
" yellow.	"	"				1						1		
For blue.	Green.	Green.												
" red.	"	"												
" green.	"	"	1			2								
" yellow.	"	"	1.			1				1				1
For blue.	Green.	Yellowish-green.												
" red.	"	"		1		1			1					
" green.	"	"		2					1	2		1. 1	1. 1	
" yellow.	"	"												
For blue.	Green.	Yellow.												1
" red.	"	"								1		1		1
" green.	"	"	3	1						1		1		1
" yellow.	"	"												

TABLE II A. NASAL MERIDIAN.—*Continued.*

Background.	Color.	Color Seen.	Degrees on the Retina.											
			81	82.5	84	85.5	87	88.5	90	91.5	92.5	94	95.5	97
For blue.	Red.	Red.			1									
" red.	"	"			2		1			1		1	1	1
" green.	"	"	1	1 1	1 1	1	1	1						
" yellow.	"	"		1		1	1							
For blue.	Red.	Orange.												
" red.	"	"	1	1										
" green.	"	"												
" yellow.	"	"												
For blue.	Red.	Yellow.			1							1		
" red.	"	"												
" green.	"	"	2											
" yellow.	"	"												
For blue.	Orange.	Orange.												
" red.	"	"											1	
" green.	"	"	1.	1.									1	
" yellow.	"	"				1		1				2		
For blue.	Orange.	Yellowish-orange.												
" red.	"	"					1							
" green.	"	"		1										
" yellow.	"	"												
For blue.	Orange.	Yellow.			1	1						1		
" red.	"	"		1	1					1	1	1		
" green.	"	"	2	1	1			1		1				
" yellow.	"	"												
For green.	Orange.	Red.												
" yellow.	"	"		2		1				1				
For blue.	Green.	Green.											1	
" red.	"	"											1	
" green.	"	"								1	1			
" yellow.	"	"		1		1		1		1				
For blue.	Green.	Yellowish-green.												
" red.	"	"				1								
" green.	"	"			1.			1						
" yellow.	"	"												
For blue.	Green.	Yellow.			1		1					1		
" red.	"	"			1				1	1		1		
" green.	"	"		2					1			1		
" yellow.	"	"												

TABLE III A. NASAL MERIDIAN.

Background.	Color.	Color Seen	Degrees on the Retina.											
			10–39	41.5	43	47	50	55	55.5	57	60.5	62	62.5	63.5
For blue.	Red.	Red.							I.		I.			
" red.	"	"							I.		I. 1			
" green.	"	"									1			
" yellow.	"	"	1	2			1		1					
For blue.	Red.	Orange.												
" red.	"	"									I.			
" green.	"	"												
" yellow.	"	"												
For blue.	Red.	Yellow.												
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For blue.	Orange.	Orange.		I.			I.		II.					
" red.	"	"	2	3			1				II.			
" green.	"	"									I.			
" yellow.	"	"	1											
For blue.	Orange.	Orange-yellow.												
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For blue.	Orange.	Yellow.												
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For green.	Orange.	Red.	1	1					1		2			1
" yellow.	"	"	5		1	2			3		1			
For blue.	Green.	Green.												
" red.	"	"												
" green.	"	"										1		
" yellow.	"	"										1		
For blue.	Green.	Greenish-yellow.												
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For blue.	Green.	Yellow.												
" red.	"	"												
" green.	"	"												
" yellow.	"	"												

TABLE III A. NASAL MERIDIAN.—*Continued.*

Background.	Color.	Color Seen.	Degrees on the Retina.											
			64.5	67	68	69	70.5	72	73	74.5	76	77	78.5	80
For blue.	Red.	Red.		I.						I.				
" red.	"	"	2	1		II.	II. 1	2	2	2	1	3 III.		1
" green.	"	"			1									
" yellow.	"	"				1		2		1		2		1
For blue.	Red.	Orange.								I.		I.		I. 1
" red.	"	"												II. 1
" green.	"	"												I.
" yellow.	"	"												
For blue.	Red.	Yellow.												
" red.	"	"												1
" green.	"	"												
" yellow.	"	"												
For blue.	Orange.	Orange.		I.				I.						
" red.	"	"	1	1									II.	
" green.	"	"		I.								I.		II.
" yellow.	"	"												2
For blue.	Orange.	Orange-yellow.												
" red.	"	"							I.			II.		1
" green.	"	"												
" yellow.	"	"					1							
For blue.	Orange.	Yellow.								1		2		1
" red.	"	"												
" green.	"	"												
" yellow.	"	"												
For green.	Orange.	Red.	I.											
" yellow.	"	"	5	1		2		2		1		3	1	1
For blue.	Green.	Green.												1
" red.	"	"												
" green.	"	"	1	1				1						1
" yellow.	"	"	1	1		1							1	
For blue.	Green.	Greenish-yellow.												
" red.	"	"								2				
" green.	"	"										1		
" yellow.	"	"												
For blue.	Green.	Yellow.												
" red.	"	"												
" green.	"	"												
" yellow.	"	"							1					

TABLE III A. NASAL MERIDIAN.—*Continued.*

Background.	Color.	Color Seen.	Degrees on the Retina.											
			81	82.5	84	85.5	87	88.5	90	91.5	92.5	94	95.5	97
For blue.	Red.	Red.		I								2		
" red.	"	"	III.							I		2		
" green.	"	"				I		2		1	1			
" yellow.	"	"		I			1		1		1			
For blue.	Red.	Orange.		I. I										
" red.	"	"		II.			I. I	I.						
" green.	"	"		I.										
" yellow.	"	"												
For blue.	Red.	Yellow.					I							
" red.	"	"			3	I	2	2						
" green.	"	"				I		I				2		
" yellow.	"	"												
For blue.	Orange.	Orange.												
" red.	"	"			1						1			
" green.	"	"				1						1		1
" yellow.	"	"		I	1	1	1		1					
For blue.	Orange.	Orange-yellow.							I.					
" red.	"	"					I							
" green.	"	"							I					
" yellow.	"	"												
For blue.	Orange.	Yellow.		I			I		I	I				
" red.	"	"				I				I				
" green.	"	"		I		I		I		3				
" yellow.	"	"								I				
For green.	Orange.	Red.												
" yellow.	"	"				2		I						
For blue.	Green.	Green.					I	I 1						
" red.	"	"		I						1				
" green.	"	"	I				2			1	1			
" yellow.	"	"												
For blue.	Green.	Greenish-yellow.												
" red.	"	"				I	I							
" green.	"	"		I		I			I					
" yellow.	"	"												
For blue.	Green.	Yellow.												
" red.	"	"					I	I						
" green.	"	"						I						
" yellow.	"	"				I								

TABLE IV A. NASAL MERIDIAN.

Background.	Color.	Color Seen.	Degrees on the Retina.																	
			10-39	41.5	43	47	50	55	55.5	57	60.5	62	62.5	63.5	64.5	67	68	69.5	70.5	72
For blue.	Red.	Red.													II.	I.		1		
" red.	"	"														1		1		1
" green.	"	"							1		I.					1		1		
" yellow.	"	"							1						4	2		3		
For blue.	Red.	Orange.									1					1 I.				
" red.	"	"																	1	
" green.	"	"																		
" yellow.	"	"																1		
For blue.	Red.	Yellow.																		
" red.	"	"																		
" green.	"	"																		
" yellow.	"	"																		
For blue.	Orange.	Orange.								I.					I.	I.				
" red.	"	"													1					
" green.	"	"													1					
" yellow.	"	"													2					
For blue.	Orange.	Orange-yellow.									I. 1				II.			1		
" red.	"	"														I.		I.		1
" green.	"	"																1		
" yellow.	"	"													1	2				
For blue.	Orange.	Yellow.														1				1
" red.	"	"																		
" green.	"	"																		
" yellow.	"	"																		
For green.	Orange.	Red.														1				
" yellow.	"	"		1			1		2		I. 1				2	2		2		
For blue.	Green.	Green.														2				
" red.	"	"														1		1	1	
" green.	"	"													1	2	1			2
" yellow.	"	"														1 2		1		
For blue.	Green.	Greenish-yellow.																1		
" red.	"	"																	1	
" green.	"	"																		
" yellow.	"	"																		
For blue.	Green.	Yellow.									1									
" red.	"	"																		
" green.	"	"																		
" yellow.	"	"																		

TABLE IV A. NASAL MERIDIAN.—*Continued.*

Background.	Color.	Color Seen.	Degrees on the Retina.																	
			73	74.5	76	77	78.5	80	81	82.5	84	85.5	87	88.5	90	91.5	92.5	94	95.5	97
For blue.	Red.	Red.			1			2		1								1		
" red.	"	"	2					3					1	1		2	1			
" green.	"	"			1		1				2		2	2	1					
" yellow.	"	"	2	1	1	1		7		2	1	2		1						
For blue.	Red.	Orange.						1			1									
" red.	"	"									2			1	1	1				
" green.	"	"											2							
" yellow.	"	"										1								
For blue.	Red.	Yellow.									1									
" red.	"	"																		
" green.	"	"																		
" yellow.	"	"																		
For blue.	Orange.	Orange.											1					1		
" red.	"	"												2					3	
" green.	"	"											1.		1		1			
" yellow.	"	"		2								1		2			1	1		
For blue.	Orange.	Orange-yellow.			1			1. 2												
" red.	"	"	1					2		1	1									
" green.	"	"			1			1			1	1	4	1						
" yellow.	"	"	1					1		1										
For blue.	Orange.	Yellow.						1		1	1		2		1					
" red.	"	"									1		1	1				2		
" green.	"	"																1		
" yellow.	"	"												1						
For green.	Orange.	Red.	1			1														
" yellow.	"	"	3		1	2		5		2	4									
For blue.	Green.	Green.																		
" red.	"	"														1	3	1		
" green.	"	"	1		1			3	1	1	1		1 1	1		1		2		
" yellow.	"	"	1 1	1	1											1				
For blue.	Green.	Greenish-yellow.	1					2			1									
" red.	"	"	2					2		1	2		1							
" green.	"	"					2					1	3	1						
" yellow.	"	"																		
For blue.	Green.	Yellow.										2								
" red.	"	"											1			1				
" green.	"	"						1					1	1	1		1			
" yellow.	"	"																		

TABLE I B. TEMPORAL MERIDIAN.

Background.	Color.	Color Seen.	Degrees on the Retina.											
			13–25	27.5	30	33	35	37	39	41.5	43	45	47	48
For blue.	Red.	Red.	6	4		2			1				1	
" green.	"	"	1				1	1	1	1 1	1			
" yellow.	"	"	7	1	3	1	1	2		2				
For blue.	Red.	Orange.				1			2 I.					
" green.	"	"				1			1					
" yellow.	"	"												
For blue.	Orange.	Orange.	3 II.	1 I.			1			2				
" green.	"	"	1	1			1		3		1 2			
" yellow.	"	"	5 R[1]		2 R	1	1?	1? R	1	1	1	2		
For blue.	Orange.	Yellowish-orange.		1		1								
" green.	"	"				1 I.	1	1	1	1				
" yellow.	"	"							1?					
For blue.	Orange.	Yellow.					1		3	1	1			
" green.	"	"							1	1	2			
" yellow.	"	"			1									
For green.	Orange.	Red.			1					1			1?	
" yellow.	"	"	1	1				1						
For blue.	Green.	Green.	5	2	1				1				1	
" green.	"	"	2					1	1?		1			
" yellow.	"	"	6		2	1		3	1?	1				
For blue.	Green.	Greenish-yellow.	1	1										
" green.	"	"	1	1		2	1	2	3		1			
" yellow.	"	"			1			1		1				
For blue.	Green.	Yellow.		2				2						
" green.	"	"												
" yellow.	"	"												

[1] R = Reddish orange.

TABLE II B. TEMPORAL MERIDIAN.

Background.	Color.	Color Seen.	Degrees on the Retina.												
			13–25	27.5	30	33	35	37	39	41.5	43	45	47	48	50
For blue.	Red.[1]	Red.	II.					2		1	I 2				
" green.	"	"							2						
" yellow.	"	"	I				1								
For blue.	Orange[1]	Orange.		2		2	I			I					
" green.	"	"													
" yellow.	"	"													
For blue.	Orange.	Orange.	I.	I.				I	1	1	1				
" green.	"	"		I.			I	I	1	1	1	1			
" yellow.	"	"		I			1		1	1					
For blue.	Orange.	Yellowish-orange.	III.		I										
" green.	"	"													
" yellow.	"	"													
For blue.	Orange.	Yellow.				I	I	2		I	I				
" green.	"	"							I		I				
" yellow.	"	"		I		I									
For green.	Orange.	Red.													
" yellow.	"	"	4		I		I?								
For blue.	Green.	Green.	I V.	.											
" green.	"	"						2	1	1					
" yellow.	"	"	2 I?	1		2	1								
For blue.	Green.	Greenish-yellow.	I.												
" green.	"	"				I	I	I							
" yellow.	"	"													
For blue.	Green.	Yellow.		2		2		3		I	I				
" green.	"	"									I				
" yellow.	"	"			I										

[1] Omitted in averages because incomplete.

TABLE III B. TEMPORAL MERIDIAN.

Background.	Color.	Color Seen.	Degrees on the Retina.													
			13–27.5	30	33	35	37	39	41.5	43	45	47		50	51	52.5
For blue.	Red.	Red.	2	2 I.			1		2		I.					2
" green.	"	"	1	1			1		2 I.		1	1	1			
" yellow.	"	"	3				2		1							
For blue.	Red.	Orange.		1							1		2			
" green.	"	"											1			
" yellow.	"	"														
For blue.	Orange.	Orange.	IV.	II.					1		1	1		1	1	1
" green.	"	"	1	·1					3		3	1				
" yellow.	"	"							1			2				
For blue.	Orange.	Yellowish-orange.	1	1 I.			1		1 II.		1	II.				
" green.	"	"									1	1				
" yellow.	"	"							1				11?			
For blue.	Orange.	Yellow.								1	1	1				
" green.	"	"										2				
" yellow.	"	"							1							
For green.	Orange.	Red.														
" yellow.	"	"	2	2			1 I.		1 I.							
For blue.	Green.	Green.	2	2			1		1	1	2	1				
" green.	"	"	1				1		3		1	1	2	1		
" yellow.	"	"	2	2			1		1	2	1	1				
For blue.	Green.	Greenish-yellow.	1?	2	1 I.											
" green.	"	"		1							2					
" yellow.	"	"														
For blue.	Green.	Yellow.							1							
" green.	"	"														
" yellow.	"	"														

TABLE I C. UPPER MERIDIAN.

Background.	Color.	Color Seen.	Degrees on the Retina.									
			10–33	35	37	39	41.5	43	45	47	48	50
For blue.	Red.	Red.	3	1			1		2			
" green.	"	"	2				3	1	1	3	1 1	2 3
" yellow.	"	"	2				1			2		3
For blue.	Red.	Orange.			1	2	1		1	2	3	3
" green.	"	"			1		1		2	3	1	1
" yellow.	"	"										
For blue.	Red.	Yellow.										
For blue.	Orange.	Orange.	2 I.		1	1	1					
" green.	"	"	2	1			3	2		2	3	2 3
" yellow.	"	"	2				1					1
For blue.	Orange.	Yellowish-orange.					1					
" green.	"	"	I.				2			1		
" yellow.	"	"										
For blue.	Orange.	Yellow.					2		1			3
" green.	"	"					1	2			1	2
" yellow.	"	"										
For green.	Orange.	Red.	3									1
" yellow.	"	"	5									
For blue.	Green.	Green.	3	1	1		1 ?					1
" green.	"	"	2 I.								1 ?	2
" yellow.	"	"	3				1					
For blue.	Green.	Greenish-yellow.			1		1					
" green.	"	"	3				2			1		
" yellow.	"	"	1									
For blue.	Green.	Yellow.	2				2	1	1	1		2
" green.	"	"					1		1	1	1	1
" yellow.	"	"										

TABLE I C. UPPER MERIDIAN.—*Continued.*

Background.	Color.	Color Seen.	Degrees on the Retina								
			51	52.5	54	55	56	57	58	59	60
For blue.	Red.	Red.						1			
" green.	"	"	I ?	2	1	1	3	1	1	1	
" yellow.	"	"	4	4 2	1 I ?	2 2	2				
For blue.	Red.	Orange.		2		I	I				
" green.	"	"									
" yellow.	"	"									
For blue.	Red.	Yellow.			I	I					
For blue.	Orange.	Orange.		1			1		1	1	
" green.	"	"	1		I		2	2		1	1
" yellow.	"	"	I	1	1	1					
For blue.	Orange.	Yellowish-orange.									
" green.	"	"			I						
" yellow.	"	"									
For blue.	Orange.	Yellow.		I	I	I	I	I			
" green.	"	"					2	I			
" yellow.	"	"									
For green.	Orange.	Red.									
" yellow.	"	"									
For blue.	Green.	Green.			1		1	1			
" green.	"	"		1	2		1				
" yellow.	"	"									
For blue.	Green.	Greenish-yellow.									
" green.	"	"	I	I							
" yellow.	"	"									
For blue.	Green.	Yellow.		I		I		I			
" green.	"	"									
" yellow.	"	"									

TABLE II C. UPPER MERIDIAN.

Background.	Color.	Color Seen.	Degrees on the Retina.							
			10–33	35	37	39	41.5	43	45	47
For blue.	Red.	Red.	2	1		1		1		
" green.	"	"			1	1	2			2
" yellow.	"	"	4					1		1
For blue.	Red.	Orange.	2	1		1	2	1		2
" green.	"	"								
" yellow.	"	"								
For blue.	Red.	Yellow.								
For blue.	Orange.	Orange.	3				1			
" green.	"	"	4		2	1	1	2 1		2
" yellow.	"	"	1							
For blue.	Orange.	Yellowish-orange.	I. 2	II.			1			
" green.	"	"			1					
" yellow.	"	"								
For blue.	Orange.	Yellow.					1	1		3
" green.	"	"								
" yellow.	"	"								
For green.	Orange.	Red.	7			1				
" yellow.	"	"	6	2		1				
For blue.	Green.	Green.	2 I.		1	1	1	1 1		
" green.	"	"	3	1			1	1		
" yellow.	"	"	2				1	1		
For blue.	Green.	Greenish-yellow.	1	2					1 I.	
" green.	"	"	3		1 ?				1	
" yellow.	"	"	1	2						
For blue.	Green.	Yellow.							1	
" green.	"	"	1			1			1	1
" yellow.	"	"								

TABLE II C. UPPER MERIDIAN.—*Continued.*

Background.	Color.	Color Seen.	Degrees on the Retina.						
			48	50	51	52.5	54	55	56
For blue.	Red.	Red.						1 1	1
" green.	"	"					1	1	1
" yellow.	"	"			1				
For blue.	Red.	Orange.				1			
" green.	"	"		2					
" yellow.	"	"							
For blue.	Red.	Yellow.				1			
For blue.	Orange.	Orange.			1	1			
" green.	"	"		1	2		1	2	
" yellow.	"	"							
For blue.	Orange.	Yellowish-orange.							
" green.	"	"							
" yellow.	"	"							
For blue.	Orange.	Yellow.							
" green.	"	"		1					
" yellow.	"	"							
For green.	Orange.	Red.							
" yellow.	"	"							
For blue.	Green.	Green.		1					
" green.	"	"			1	1		2	1
" yellow.	"	"							
For blue.	Green.	Greenish-yellow.							
" green.	"	"					1		
" yellow.	"	"							
For blue.	Green.	Yellow.							
" green.	"	"							
" yellow.	"	"							

Table III c. Upper Meridian.

Background.	Color.	Color Seen.	Degrees on the Retina.																		
			3–33	35	37	39	41.5	43	45	47	48	50	51	52½	54	55	56	57	58	59	60
For blue.	Red.	Red.	3 I.			1	1	1			1										
" green.	"	"	1		1		2	1				2		1	2	2 **1**	**2** 1				
" yellow.	"	"	1	1	2	2	3 **1**	2	1 **1**	1 **2**	1	1	**1**		1	1 1	1 **2**				
For blue.	Red.	Orange.			1	1															
For blue.	Orange.	Orange.	III. 1	I.	1?																
" green.	"	"	4		2						1										**2** 1
" yellow.	"	"	1		1	2				**1**			**1**				**1**				
For blue.	Orange.	Yellowish-orange.				II.	1 I.	1				I.									
" green.	"	"										1			1		1			1	2
" yellow.	"	"																			
For green.	Orange.	Red.	2	1			1					1		1			1				
" yellow.	"	"	2		2	2	2		1	1			2	1		1					
For blue.	Green.	Green.	2 I.		1**1**	1	**1**	1 **1**				**1**									
" green.	"	"	4	**1**		**1**	**1**						**1**	**1**	**2**						
" yellow.	"	"	2					**1**													
For blue.	Green.	Greenish-yellow.	2	2						1 I.											
" green.	"	"	4		1?					1					1						
" yellow.	"	"	1	2																	
For blue.	Green.	Yellow.								1											
" green.	"	"	1			1				1	1										
" yellow.	"	"	2																		

Table I d. Lower Meridian.

Background.	Color.	Color Seen.	Degrees on the Retina.											
			13-22	25	27.5	30	33	35	37	39	41.5	43	45	47
For blue.	Red.	Red.			1	1								
" green.	"	"	3	2		1	3	2 **3**	**1**	**2**				
" yellow.	"	"					1	1	1	**1**				
For blue.	Red.	Orange.			1		1	2						
" green.	"	"												
" yellow.	"	"					1							
For blue.	Orange.	Orange.			1	I.			1			**1**	**1**	
" green.	"	"	3	2				1	2	**2**	**2**			
" yellow.	"	"						1			1	**1**	**1**	
For blue.	Orange.	Yellowish-orange.					1	1 I.		1	1			
" green.	"	"												
" yellow.	"	"												
For green.	Orange.	Red.	1		1			1		1				
" yellow.	"	"	2 I.	I.	1		1							
For blue.	Green.	Green.	1					**1**						
" green.	"	"	1				**1**		**1**		**1**			
" yellow.	"	"	2	1	1?		**2**							
For blue.	Green.	Greenish-yellow.	1				1	1						
" green.	"	"	2		1	1	1	1						
" yellow.	"	"												
For blue.	Green.	Yellow.						1	1					
" green.	"	"							1	1				
" yellow.	"	"												

TABLE II D. Lower Meridian.[1]

Background.	Color.	Color Seen.	Degrees on the Retina.											
			13-22	25	27.5	30	33	35	37	39	41.5	43	45	47
For blue.	Red.	Red.	2 1.	1								1		
" green.	"	"			1		1		2		1	1		
" yellow.	"	"		1	1				1		1			
For blue.	Red.	Orange.					1		1	1	1			
" green.	"	"												
" yellow.	"	"												
For blue.	Red.	Yellow.									2	1?		
For blue.	Orange.	Orange.	2 1							1		1	1	
" green.	"	"	2	1		1			1		1		1	
" yellow.	"	"				1								
For blue.	Orange.	Yellowish-orange.					1		1					
" green.	"	"							1					
" yellow.	"	"	1											
For blue.	Orange.	Yellow.										1		
" green.	"	"							1	1				
" yellow.	"	"					1							
For green.	Orange.	Red.												
" yellow.	"	"		1	1		1							
For blue.	Green.	Green.	1					1		1				
" green.	"	"	1											
" yellow.	"	"												
For blue.	Green.	Greenish-yellow.	1.											
" green.	"	"												
" yellow.	"	"												
For blue.	Green.	Yellow.	1		1		1		1					
" green.	"	"			1		1		2					
" yellow.	"	"												

[1] For Table III D, see p. 406.

EDITORS' ANNOUNCEMNT.

His colleagues regret that stress of other duties compels Professor Warren to relinquish the duties of the position of Business Manager of the REVIEW publications. In his place Dr. J. W. Baird, of the Johns Hopkins University, will assume the Business Management. We append a statement of the present location of the responsible bureaus of the REVIEW, with the appropriate addresses for the various sorts of communication.

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